ON A FAMILIAR THEOREM OF THE THEORY OF FUNCTIONS.

BY PROFESSOR EDMUND LANDAU.

In the second part of a paper* published in 1896, Professor Osgood considers the theorem:

"Let a function $f(z)$ of the complex variable z be single-valued and analytic at all points of the neighborhood of the point $z = c$ (exclusive of $z = c$), that is, for $0 < |z - c| < \gamma$; also, for these values of z, let $|f(z)|$ remain less than some fixed value. Then $\lim_{z=c} f(z) = \alpha$ exists, and if at $z = c$ we assign to the function the value α, then $f(z)$ will be analytic at the point c."

This follows easily, for example, from Laurent's theorem. Professor Osgood mentions the various proofs to be found in the literature of the subject and states that in several treatises the following fallacious proof is contained:

Let a function $\phi(z)$ be defined by the equations

$$\phi(z) = (z - c)f(z) (0 < |z - c| < \gamma), \quad \phi(c) = 0;$$

then by hypothesis, $\lim_{z=c} \phi(z) = 0$, and $\phi(z)$ is continuous at the point c and analytic for $0 < |z - c| < \gamma$. From this some writers incorrectly infer that $\phi(z)$ is also analytic at $z = c$.

This conclusion is not justified, even when the existence of $\lim_{z=c} f(z) = \alpha$, or of

$$\phi'(c) = \lim_{z=c} \frac{(z - c)f(z)}{z - c} = \alpha \tag{1}$$

is presupposed.† For from the existence of the derivative of $\phi(z)$, without knowledge of its continuity, it could not be inferred, previously to 1900, that $\phi(z)$ is analytic. Not until Goursat's ‡ proof of Cauchy's integral theorem had appeared

* "Some points in the elements of the theory of functions." BULLETIN, ser. 2, vol. 2, pp. 296–302.

† This assumption is no restriction of the generality. For the function $(z - c)f(z)$ has a limiting value, namely zero, for $z = c$, and it is sufficient to show that this function is analytic at $z = c$.

‡ "Sur la définition générale des fonctions analytiques d'après Cauchy." *Trans. Amer Math. Society*, vol 1 (1900), pp. 14–16. Compare the more correct presentation of Goursat's proof by Pringsheim. "Ueber den Goursatschen Beweis des Cauchyschen Integralsatzes," ibid, vol. 2 (1901), pp. 413–421.

was this conclusion, to which Professor Osgood quite rightly took exception in 1896, later justified.

At the end of his paper, Professor Osgood has added to the valid proofs of the theorem two new demonstrations.

I should like to communicate another proof, which stands in closer relation to the fallacious process of thought previously referred to, and which permits the gap to be filled without the use of Goursat's theorem.

There was lacking only the proof that the derivative $\phi'(z)$ is continuous at the point $z = c$. Since for $0 < |z - c| < \gamma$ we have

$$\phi'(z) = (z - c)f'(z) + f(z), \tag{2}$$

and since the existence of

$$\lim_{z=c} f(z) = \alpha \tag{3}$$

might be assumed, it only remains to show that

$$\lim_{z=c} (z - c)f'(z)$$

exists and is equal to zero.

From (3), there exists for any previously chosen δ an ϵ such that for $0 < |z - c| < \epsilon$ we have $|f(z) - \alpha| < \delta$. If we take $0 < |z - c| < \frac{2}{3}\epsilon$, and apply Cauchy's formula to the function $f(z) - \alpha$, we get

$$f'(z) = \frac{1}{2\pi i}\int \frac{f(x) - \alpha}{(x - z)^2}\, dx,$$

where the integral is extended over the circumference with center at z and radius $|z - c|/2$. Hence follows

$$|f'(z)| < \frac{1}{2\pi} \cdot 2\pi \frac{|z - c|}{2} \cdot \frac{\delta}{\frac{|z - c|^2}{4}} = \frac{2\delta}{|z - c|},$$

$$|(z - c)f'(z)| < 2\delta,$$

or

$$\lim_{z=c} (z - c)f'(z) = 0$$

and, by use of (1), (2) and (3),

$$\lim_{z=c} \phi'(z) = \alpha = \phi'(c).$$

BERLIN,
August 9, 1905.

RATIONAL PLANE CURVES RELATED TO RIEMANN TRANSFORMATIONS.

BY PROFESSOR H. S. WHITE.

(Read before the American Mathematical Society, September 8, 1905.)

In a plane considered as filled with algebraic curves of all orders and deficiencies, a perfectly arbitrary net or doubly infinite linear system of such curves may be regarded as effecting a one-to-one transformation. For exceptional curves the transformation may be two-to-one (or m-to-one), and such curves are termed in another connection involution curves of the net. Only in case the net consists of rational curves, having but a single variable point of intersection is the transformation a Cremona transformation. In other cases, barring all involution curves of the net, it is called a Riemann transformation, or a birational transformation of the curve under consideration. Let the x-plane be transformed into the y-plane by the substitution

$$\rho y_1 = \phi_1(x_1, x_2, x_3) = \phi_1(x), \quad \rho y_2 = \phi_2(x), \quad \rho y_3 = \phi_3(x);$$

and let $f(x) = 0$ represent a curve which is transformed into $F(y) = 0$. To every point (y) on the curve F (with a finite number of exceptions) let only one point (x) correspond, then the transformation is birational for that curve; and we may call it a Riemann transformation of the plane, as proposed by Dr. Kasner. If the curves $\phi_1(x) = 0$, $\phi_2(x) = 0$, $\phi_3(x) = 0$ are rational of order n, with multiple points of like structure at fixed points, and enough other fixed simple base points to leave only one intersection variable, then we have a Cremona transformation of the entire plane. My purpose is to call attention to a peculiarity of rational curves under Riemann transformations, viz., to show that each possesses, for every Riemann transformation, one of the Cremona type which is on that curve equivalent to the former.

Begin with a straight line U, or $(ux) = 0$. Replace the Riemann substitution by the following:

$$\rho y_1 = \phi_1(x) + \psi_1(x) \cdot (ux) \equiv \Phi_1(x),$$
$$\rho y_2 = \phi_2(x) + \psi_2(x) \cdot (ux) \equiv \Phi_2(x),$$
$$\rho y_3 = \phi_3(x) + \psi_3(x) \cdot (ux) \equiv \Phi_3(x),$$

and take ψ_1, ψ_2, ψ_3 any forms of order $n-1$ to make the expressions on the right homogeneous. For every point x on $(ux)=0$ this gives the same point (y) as before, *i. e.*, it is equivalent to the former on the line U. The net of Φ-curves can now be made a Cremona net of Jonquières type. For each ψ contains $\frac{1}{2}\,n\,(n+1)$ constants linearly. To have a point of multiplicity $n-1$ in a specified point O is equivalent to $\frac{1}{2}(n-1)n$ linear conditions. Each ψ retains now n parameters: select those arbitrarily for one ψ, say ψ_1, then dispose of the remaining $2n$ so that the curves $\Phi_2=0$ and $\Phi_3=0$ may have on $\Phi_1=0$ $2n-2$ additional points of intersection in common, none of them on the line U. The transformation is now of the Cremona type, as was required.

On every straight line, a Riemann transformation is conformable with an indefinitely great number of Cremona transformations.

Next consider any rational curve of order above the first, $f(x)=0$. Find a Cremona transformation S which converts f into the line U, and let S^{-1} denote its inverse. Let R denote the proposed Riemann transformation, call $SR \equiv R'$, and by the above theorem R' must be conformable along the line U to a Cremona transformation K. Then along the curve f, we must have $S^{-1}SR$ or $S^{-1}R'$ conformable with $S^{-1}K$ or K'; and evidently this is of the Cremona type, since Cremona transformations constitute a group. This shows that:

On any rational plane curve, not an involution curve of the net, a Riemann transformation R with a net of φ-curves is equivalent to or conformable with a Cremona transformation whose net of Φ-curves is of the Jonquières type.

Remembering that a rational curve has no invariants (or moduli) under algebraic transformation, we have learned that also it can have none under Cremona transformations; or negatively stated, the distinctive properties of the Cremona group within the group of Riemann transformations must be sought in their effect on systems of curves or on single curves of deficiency higher than zero.

The extension of this remark to space of more than two dimensions is not apparent. For verification, nets of conics and cubics have been employed, and no obstacle found to the actualization of the steps referred to in the above counting of constants.

EVANSTON, ILLINOIS,
June, 1905.

ON LAMÉ'S SIX EQUATIONS CONNECTED WITH TRIPLY ORTHOGONAL SYSTEMS OF SURFACES.

BY J. E. WRIGHT, M.A.

LAMÉ* has shown for a triply orthogonal system of surfaces given by the parameters ρ, ρ_1, ρ_2 that if the square of the element of length is given by $ds^2 = H^2 d\rho + H_1^2 d\rho_1^2 + H_2^2 d\rho_2^2$, where H, H_1, H_2 are certain functions of ρ, ρ_1, ρ_2, then H, H_1, H_2 must satisfy the following system of equations:

$$\frac{\partial^2 H}{\partial \rho_1 \partial \rho_2} = \frac{1}{H_1}\frac{\partial H}{\partial \rho_1}\frac{\partial H_1}{\partial \rho_2} + \frac{1}{H_2}\frac{\partial H}{\partial \rho_2}\frac{\partial H_2}{\partial \rho_1} \qquad (1)$$

and two others of the same type; (2), (3)

$$\frac{\partial}{\partial \rho_1}\left(\frac{1}{H_1}\frac{\partial H}{\partial \rho_1}\right) + \frac{\partial}{\partial \rho}\left(\frac{1}{H}\frac{\partial H_1}{\partial \rho}\right) + \frac{1}{H_2^2}\frac{\partial H}{\partial \rho_2}\frac{\partial H_1}{\partial \rho_2} = 0 \qquad (4)$$

with two others of this type. (5), (6)

Also if V is a function of x, y, z for which

$$\frac{\partial^2 V}{\partial x^2} + \frac{\partial^2 V}{\partial y^2} + \frac{\partial^2 V}{\partial z^2} = 0,$$

he has shown that

$$\frac{\partial}{\partial \rho}\left(\frac{H_1 H_2}{H}\frac{\partial V}{\partial \rho}\right) + \frac{\partial}{\partial \rho_1}\left(\frac{H_2 H}{H_1}\frac{\partial V}{\partial \rho_1}\right) + \frac{\partial}{\partial \rho_2}\left(\frac{H H_1}{H_2}\frac{\partial V}{\partial \rho_2}\right) = 0.$$

If the system of coordinates ρ, ρ_1, ρ_2 is isothermal, this equation must be satisfied by $V = \rho$, or by $V = \rho_1$, or by $V = \rho_2$. Hence $H_1 H_2 / H = Q^2$, where Q is a function of ρ_1 and ρ_2 only. Similarly $H_2 H / H_1 = Q_1^2$, and $H H_1 / H_2 = Q_2^2$, where Q_i is a function not involving the variable ρ_i. Hence $H = Q_1 Q_2$, $H_1 = Q_2 Q$, $H_2 = Q Q_1$. The six equations given above transform into six in the variables Q. Lamé † gives a solution of

* Leçons sur les coordonées curvilignes (1859), pp. 76, 78.

† Loc. cit., p. 99.

these equations by first finding a solution of equations (1), (2), (3) and using this to solve the remainder. He makes the statement that his solution of the first three equations is the most general possible *; this statement is obviously inaccurate and it seems of interest to give a complete solution of the equations. The solution of the first three equations, or rather a comparison of two different solutions, leads to a curious result in the theory of elimination.

Equation (1) becomes

$$Q\frac{\partial Q_2}{\partial \rho_1}\frac{\partial Q_1}{\partial \rho_2} = Q_1\frac{\partial Q_2}{\partial \rho_1}\frac{\partial Q}{\partial \rho_2} + Q_2\frac{\partial Q_1}{\partial \rho_2}\frac{\partial Q}{\partial \rho_1}, \tag{7}$$

with similar expressions for (2) and (3). Multiply (7) by $\partial Q_1/\partial \rho$, and the transformed expression for (3) by $\partial Q_1/\partial \rho_2$, and add. The result is

$$Q_1\left[\frac{\partial Q_2}{\partial \rho}\frac{\partial Q}{\partial \rho_1}\frac{\partial Q_1}{\partial \rho_2} + \frac{\partial Q_1}{\partial \rho}\frac{\partial Q_2}{\partial \rho_1}\frac{\partial Q}{\partial \rho_2}\right] = 0.$$

Hence unless all the Q's vanish identically

$$\frac{\partial Q_2}{\partial \rho}\frac{\partial Q}{\partial \rho_1}\frac{\partial Q_1}{\partial \rho_2} + \frac{\partial Q_1}{\partial \rho}\frac{\partial Q_2}{\partial \rho_1}\frac{\partial Q}{\partial \rho_2} = 0. \tag{8}$$

Assume that none of the derivatives in (8) vanish identically and write

$$K = -\frac{\partial Q}{\partial \rho_1}\Big/\frac{\partial Q}{\partial \rho_2}, \quad K_1 = -\frac{\partial Q_1}{\partial \rho_2}\Big/\frac{\partial Q_1}{\partial \rho}, \quad K_2 = -\frac{\partial Q_2}{\partial \rho}\Big/\frac{\partial Q_2}{\partial \rho_1}.$$

Equation (8) becomes $KK_1K_2 = 1$, where K_i is a function not involving ρ_i. By taking logarithms and differentiating with respect to two of the variables ρ, it is easy to prove that the most general values for the K's are

$$K = \frac{a_2}{a_1}, \quad K_1 = \frac{a}{a_2}, \quad K_2 = \frac{a_1}{a},$$

where a is a function of ρ only and similarly a_1 and a_2 are functions of ρ_1 and ρ_2 alone respectively.

* Loc. cit., p. 100, line 23.

If we put $\int a d\rho = \sigma$, with similar values for σ_1 and σ_2, we deduce that Q is a function of $\sigma - \sigma_2$ only, Q_1 a function of $\sigma_2 - \sigma$ only, and Q_2 a function of $\sigma - \sigma_1$ only. Substitute these values in (7) and it readily reduces to

$$\frac{Q}{Q'} + \frac{Q_1}{Q_1'} + \frac{Q_2}{Q_2'} = 0.$$

Hence

$$\frac{Q}{Q'} = n(\sigma_1 - \sigma_2), \quad \frac{Q_1}{Q_1'} = n(\sigma_2 - \sigma), \quad \frac{Q_2}{Q_2'} = n(\sigma - \sigma_1),$$

where n is a constant, and therefore

$$Q = c(\sigma_1 - \sigma_2)^{1/n}, \quad Q_1 = c_1(\sigma_2 - \sigma)^{1/n}, \quad Q_2 = c_2(\sigma - \sigma_1)^{1/n}, \quad (A)$$

where c, c_1, c_2 are constants. This is the most general solution, provided none of the derivatives in (8) vanish. If, however, for example, $\partial Q_2/\partial \rho_1 = 0$, then either $\partial Q_2/\partial \rho = 0$, or $\partial Q/\partial \rho_1 = 0$, or $\partial Q_1/\partial \rho_2 = 0$. Equation (7), however, shows that $\partial Q_2/\partial \rho_1 = 0$ implies either $\partial Q/\partial \rho_1 = 0$ or $\partial Q_1/\partial \rho_2 = 0$. $\partial Q_2/\partial \rho_1 = 0$, $\partial Q_1/\partial \rho_2 = 0$ lead to the solution

$$Q_1 = f(\rho), \quad Q_2 = cf(\rho), \quad Q = \phi(\rho_1 \rho_2), \qquad (B)$$

where f and ϕ are arbitrary functions of their arguments and c is a constant. $\partial Q_2/\partial \rho_1 = 0$, $\partial Q/\partial \rho_1 = 0$ lead to the solution

$$Q_2 = f(\rho), \quad Q = \phi(\rho_2), \quad Q_1 = F(Q, Q_2), \qquad (C)$$

where F is homogeneous and of unit degree in Q_1, Q_2. These types (A), (B), (C) are the only three types of solution.

We now proceed to the integration of the equations in a different manner. Write $\log Q = \lambda$, $\log Q_1 = \lambda_1$, $\log Q_2 = \lambda_2$. Equations (1), (2), (3) become

$$\frac{\partial \lambda_2}{\partial \rho_1}\frac{\partial \lambda_1}{\partial \rho_2} = \frac{\partial \lambda_2}{\partial \rho_1}\frac{\partial \lambda}{\partial \rho_2} + \frac{\partial \lambda}{\partial \rho_1}\frac{\partial \lambda_1}{\partial \rho_2} \qquad (9)$$

and two similar equations.

Write $\lambda_1 - \lambda_2 = \omega$, $\lambda_2 - \lambda = \omega_1$, $\lambda - \lambda_1 = \omega_2$ and (9) becomes

$$\frac{\partial \omega_1}{\partial \rho_1}\frac{\partial \omega_2}{\partial \rho_2} - \frac{\partial \omega_1}{\partial \rho_2}\frac{\partial \omega_2}{\partial \rho_1} = 0,$$

or

$$J\begin{pmatrix} \omega_1 & \omega_2 \\ \rho_1 & \rho_2 \end{pmatrix} = 0.$$

Hence the equation implies the existence of a relation, $f(\omega_1, \omega_2, \rho) = 0$. Exactly similarly there are relations

$$f_1(\omega_2, \omega, \rho_1) = 0, \quad f_2(\omega, \quad \omega_1, \rho_2) = 0.$$

From these equations, together with $\omega + \omega_1 + \omega_2 = 0$, it is easy to deduce that either ρ, ρ_1, ρ_2 do not any of them occur explicitly in f, f_1, f_2 or if, for example, ρ occurs in f, it is easy to prove that either ρ_1 is absent from f_1, or ρ_2 from f_2. The latter case is thus reduced to the former, for if one relation of the type $f(\omega, \omega_1) = 0$ exist, then two others of that type also exist in virtue of the relation $\omega + \omega_1 + \omega_2 = 0$.

Substituting for the ω's in terms of the Q's, we immediately deduce that the solution is equivalent to the statement that a homogeneous relation $F(Q, Q_1, Q_2) = 0$ exists among the Q's.

Combining the two solutions we have the following theorem connected with the theory of elimination:

Let $F(Q, Q_1, Q_2) = 0$ *be any homogeneous relation. It is possible to express* Q *as a function of two variables* ρ_1 *and* ρ_2, Q_1 *as a function of two variables* ρ_2 *and* ρ, *and* Q_2 *as a function of the variables* ρ *and* ρ_1, *in two cases only*:

(*A*) *If* F *is of the form* $aQ^n + a_1 Q_1^n + a_2 Q_2^n$, *where* a, a_1, a_2 *are constants.**

(*C*) *If* F *is general, and e. g.* Q *is a function of* ρ_1 *only, and* Q_1 *a function of* ρ *only.*

It is not difficult to complete the solution of the equations (1), ⋯, (6). It may readily be shown that for case (*A*) (4), (5), and (6) are not satisfied unless $n = \frac{1}{2}$, and then

$$\sigma = A\wp\left(\frac{\rho + a}{\sqrt{c}}, g_2, g_3\right) + B,$$

$$\sigma_1 = A\wp\left(\frac{\rho_1 + a_1}{\sqrt{c_1}}, g_2, g_3\right) + B,$$

* This includes (*B*).

$$\sigma_2 = A\wp\left(\frac{\rho_2 + a_2}{\sqrt{c_2}}, g_2, g_3\right) + B,$$

where the a's, the g's, A and B are constants.

For case (c) the complete solution is

$$Q_1 = a \operatorname{cosec}(b\rho_2 + c), \quad Q_2 = a' \operatorname{cosec}(b'\rho_1 + c'),$$
$$Q = A[\operatorname{cosec}^2(b\rho_2 + c) - \operatorname{cosec}^2(b'\rho_1 + c')]^{\frac{1}{2}},$$

where A, a, a', b, b', c, c' are constants such that

$$a^2b'^2 + a'^2b^2 = 0.$$

In case (B)

$$Q_1 = \frac{1}{a\rho + b}, \quad Q_2 = \frac{k}{a\rho + b}$$

where a, b, k are constants, and Q is a function of ρ_1 and ρ_2 which satisfies the equation

$$\left(k^2\frac{\partial^2}{\partial\rho_2^2} + \frac{\partial^2}{\partial\rho_1^2}\right)\log Q + a^2Q^2 = 0.$$

Of these three types of solution, the first is the same as that given by Lamé.* He gives it in different form, and his method of obtaining it is different. He falls into the error of imagining that the most general solution of the *first* three of his equations corresponds to the case of $n = \frac{1}{2}$, and it happens that this error is largely corrected because the *second* three equations require this limitation in case (A); case (B), however, escapes his notice.

The surfaces corresponding to the three solutions are readily obtained. (A) gives a system of confocal quadrics, and (C) a system of confocal spheroids with their axial planes. (B) gives a system of concentric spheres, and the conical surfaces obtained by joining the common centre to any set of isothermal lines on one of them.

BRYN MAWR,
October, 1905.

* Loc. cit., p. 104.

CERTAIN SURFACES ADMITTING OF CONTINUOUS DEFORMATION WITH PRESERVATION OF CONJUGATE LINES.

BY DR. BURKE SMITH.

(Read before the American Mathematical Society, December 30, 1904.)

BIANCHI has shown* that surfaces which admit of continuous deformation with preservation of conjugate lines are the associates of surfaces whose curvature in terms of the parameters of the asymptotic lines is of the form $K = -[\phi(u)+\chi(v)]^{-2}$, where ϕ and χ are arbitrary. Stäckel † has considered those surfaces on which the two families of lines of curvature fall together into a single family which are also at the same time minimal lines and asymptotic lines. The curvature of such surfaces may be written in the form $K = e^{-2m(v)}$ and consequently it is of the above type. We proceed to find the coordinates x_0, y_0, z_0 of certain of these surfaces.

The fundamental magnitudes of the Gauss sphere which corresponds to such a surface may be written

$$(1)\quad e = 0,\quad f = \frac{-2\phi'(u)\psi'(v)}{[\phi(u)+\psi(v)]^2},\quad g = \frac{-2m'(v)\psi'(v)}{\phi(u)+\psi(v)} + n(v),$$

where the primes denote derivatives and ϕ, ψ, m and n are arbitrary. We consider only the case where $\phi \equiv u$ and $\psi \equiv v$. Upon substituting in the fundamental equations for the second derivatives of X and the first derivatives of x with regard to u and v we have

$$(2)\qquad \frac{\partial^2 X}{\partial u^2} = \frac{-2}{u+v}\frac{\partial X}{\partial u},$$

$$(3)\qquad \frac{\partial^2 X}{\partial u \partial v} = \frac{-m'}{2}\frac{\partial X}{\partial u} + \frac{2}{(u+v)^2}X,$$

$$(4)\qquad \frac{\partial^2 X}{\partial v^2} = \begin{Bmatrix} 2 & 2 \\ 1 & \end{Bmatrix}'\frac{\partial X}{\partial u} + \tfrac{1}{2}\left[m' - \frac{4}{u+v}\right]\frac{\partial X}{\partial v} + \left[\frac{2m'}{u+v} - n\right]X,$$

* Bianchi-Lukat : Vorlesungen über Differential-Geometrie, p. 337.
† *Leipziger Ber.*, vol. 48, p. 479.

$$(5)\qquad \begin{cases} \dfrac{\partial x_0}{\partial u} = -e^m \dfrac{\partial X}{\partial u}, \\ \dfrac{\partial x_0}{\partial v} = \dfrac{u+v}{2}\,[2m' - (u+v)n]e^m \dfrac{\partial X}{\partial u} - e^m \dfrac{\partial X}{\partial v}, \end{cases}$$

with similar equations for Y, Z, y_0, z_0, where

$$\begin{Bmatrix} 2 & 2 \\ 1 & \end{Bmatrix}' = \tfrac{1}{4}\big(2(u+v)(m'' - m'^2) + m'[6 + (u+v)^2 n] - (u+v)[(u+v)n' + 4n]\big).$$

The general integral of (2) is

$$(6)\qquad X = \frac{F(v)}{u+v} + G(v).$$

Now (3) has equal invariants, and by the substitution $\theta = X\sqrt{ie^m}$ becomes $\dfrac{\partial^2\theta}{\partial u\,\partial v} = \dfrac{2\theta}{(u+v)^2}$, of which the general integral is

$$\theta = \frac{2(U_1 + V_1)}{u+v} - U_1' - V_1',$$

U_1 and V_1 being arbitrary functions of u and v respectively. Comparing with (6), we must have $U_1 \equiv 0$ and X can be written in the form

$$X = \frac{2V}{u+v} - V' - \frac{m'}{2}V.$$

Since also (4) must be satisfied, we have to determine V the equation

$$(7)\quad V''' + \left(m'' - \frac{m'^2}{4} + n\right)V' + \left(\frac{m'''}{2} - \frac{m''m'}{4} + \frac{n'}{2}\right)V = 0,$$

where the primes denote derivatives. To every solution of (7) we have a value for X, and similar equations hold for Y and Z. When $n = m'^2/4 - m'' + 1$, (7) takes the form $V''' + V' = 0$ and consequently, $V = a\cos v + b\sin v + c$. If the constants a, b, c, are so chosen that $X^2 + Y^2 + Z^2 = 1$ then (5) will give by quadrature the coördinates x_0, y_0, z_0. Take, for example, a, b, c, so that

$$X=\left(\frac{2}{u+v}-\frac{m'}{2}\right)\cos v+\sin v,$$

$$Y=\left(\frac{2}{u+v}-\frac{m'}{2}\right)\sin v-\cos v, \qquad Z=i\left(\frac{2}{u+v}-\frac{m'}{2}\right).$$

Then from (5) we have

$$(8)\begin{cases} x_0=\dfrac{-2e^m\cos v}{u+v}+\displaystyle\int e^m\left[\left(\frac{m'^2}{4}-\frac{m''}{2}\right)\cos v-\frac{m'}{2}\sin v\right]dv \\ y_0=\dfrac{-2e^m\sin v}{u+v}+\displaystyle\int e^m\left[\left(\frac{m'^2}{4}-\frac{m''}{2}\right)\sin v+\frac{m'}{2}\cos v\right]dv \\ iz_0=\dfrac{2e^m}{u+v}-\displaystyle\int e^m\left(\frac{m'^2}{4}-\frac{m''}{2}+1\right)dv, \end{cases}$$

where $m=m(v)$ may be chosen arbitrarily.

In another place the writer has shown that the equations of the surfaces $S(x, y, z)$ which are associate to a given surface $S_0(x_0, y_0, z_0)$ may be written in the form

$$(9)\qquad \begin{cases} x=\rho^{-\frac{1}{2}}(eg-f^2)^{-\frac{1}{4}}\left(\theta\dfrac{\partial^2x_0}{\partial u\partial v}-\dfrac{\partial\theta}{\partial u}\dfrac{\partial x_0}{\partial v}-\dfrac{\partial\theta}{\partial v}\dfrac{\partial x_0}{\partial u}\right), \\ y=\rho^{-\frac{1}{2}}(eg-f^2)^{-\frac{1}{4}}\left(\theta\dfrac{\partial^2y_0}{\partial u\partial v}-\dfrac{\partial\theta}{\partial u}\dfrac{\partial y_0}{\partial v}-\dfrac{\partial\theta}{\partial v}\dfrac{\partial y_0}{\partial u}\right), \\ z=\rho^{-\frac{1}{2}}(eg-f^2)^{-\frac{1}{4}}\left(\theta\dfrac{\partial^2z_0}{\partial u\partial v}-\dfrac{\partial\theta}{\partial u}\dfrac{\partial z_0}{\partial v}-\dfrac{\partial\theta}{\partial v}\dfrac{\partial z_0}{\partial u}\right), \end{cases}$$

where $-1/\rho^2$ is the curvature of S_0 and θ is a solution of the equation

$$\frac{\partial^2\theta}{\partial u\partial v}=\left(-\frac{1}{4}\frac{\partial\rho}{\partial u}\frac{\partial\rho}{\partial v}-f\right)\theta.$$

This equation becomes, in the case of the surfaces defined by (8), $\dfrac{\partial^2\theta}{\partial u\partial v}=\dfrac{2\theta}{(u+v)^2}$, of which the general solution is

$$\theta=\frac{2(U+V)}{u+v}-U'-V'.$$

On substituting in (9), we have

$$x = e^{-m/2}(u+v)^2\left\{\left[A\left(\frac{m'^2}{4}-\frac{m''}{2}\right)+Bm'+D\right]\cos v - \left(A\frac{m'}{2}+B\right)\sin v\right\},$$

$$y = e^{-m/2}(u+v)^2\left\{\left[A\left(\frac{m'^2}{4}-\frac{m''}{2}\right)+Cm'+D\right]\sin v + \left(A\frac{m'}{2}+C\right)\cos v\right\},$$

$$-iz = e^{-m/2}(u+v)^2\left\{A\left(\frac{m'^2}{4}-\frac{m''}{2}+1\right)+Cm'+D\right\},$$

where

$$A = \frac{U+V}{(u+v)^2}-\frac{U'}{u+v}+\frac{U''}{2}, \quad B = \frac{U'-V'}{(u+v)^2}+\frac{U''}{u+v},$$

$$C = \frac{U'-V'}{(u+v)^2}-\frac{U''}{u+v}, \qquad D = \frac{U''+V''}{(u+v)^2}.$$

These equations, depending on three arbitrary functions, U, V and m, are therefore the equations of a group of surfaces which admit of continuous deformation with preservation of conjugate lines.

In the special case where $m = 0$, equations (8) reduce to

$$(10)\quad x_0 = \frac{-2\cos v}{u+v}, \quad y_0 = \frac{-2\sin v}{u+v}, \quad iz_0 = \frac{2}{u+v}-v,$$

which are the equations of a ruled surface whose generatrices are straight lines of zero length. The equations of the corresponding associated surfaces are

$$(11)\begin{cases} x = (U''+V'')\cos v - (U'-V'+(u+v)U''), \\ y = (U''+V'')\sin v + (U'-V'-(u+v)U''), \\ -iz = U''+V''+U+V-(u+v)U'+\dfrac{(u+v)^2}{2}U''. \end{cases}$$

The following relations exist between the symbols of Christoffel formed for any surface S_0 and its associate surface S and the Gauss sphere, provided S_0 is referred to its asymptotic lines and S is referred to conjugate lines : *

$$\begin{gathered}\left\{\begin{smallmatrix}1&2\\1\end{smallmatrix}\right\}_{S_0}=-\left\{\begin{smallmatrix}1&2\\1\end{smallmatrix}\right\}'=\frac{D''}{D}\left\{\begin{smallmatrix}1&1\\2\end{smallmatrix}\right\}_S,\quad \left\{\begin{smallmatrix}1&2\\2\end{smallmatrix}\right\}_{S_0}=-\left\{\begin{smallmatrix}1&2\\2\end{smallmatrix}\right\}'=\frac{D}{D'}\left\{\begin{smallmatrix}2&2\\1\end{smallmatrix}\right\}_S,\\ (12)\\ \left\{\begin{smallmatrix}2&2\\1\end{smallmatrix}\right\}_{S_0}=-\left\{\begin{smallmatrix}2&2\\1\end{smallmatrix}\right\}'=\frac{D''}{D}\left\{\begin{smallmatrix}1&2\\2\end{smallmatrix}\right\}_S,\quad \left\{\begin{smallmatrix}1&1\\2\end{smallmatrix}\right\}_{S_0}=-\left\{\begin{smallmatrix}1&1\\2\end{smallmatrix}\right\}'=\frac{D}{D'}\left\{\begin{smallmatrix}1&2\\1\end{smallmatrix}\right\}_S.\end{gathered}$$

If we exclude developable surfaces, the second fundamental magnitudes D and D'' of S cannot be identically zero. Now by (1) the fundamental magnitudes of the sphere which corresponds to (10) and (11) are,

$$e=0,\quad f=\frac{-2}{(u+v)^2},\quad g=1.$$

Hence in this case, $\left\{\begin{smallmatrix}1&2\\1\end{smallmatrix}\right\}'=\left\{\begin{smallmatrix}1&2\\2\end{smallmatrix}\right\}'=\left\{\begin{smallmatrix}1&1\\2\end{smallmatrix}\right\}'=0$. Thus taking account of (1) and (12), we have for (11), $\left\{\begin{smallmatrix}1&1\\2\end{smallmatrix}\right\}=0$ and $E\neq 0$. Hence the lines $v=$ const. on (11) are geodesics. Also $\left\{\begin{smallmatrix}2&2\\1\end{smallmatrix}\right\}=0$ and $G=0$ and hence the lines $u=$ const. are minimal lines. Thus the surfaces (11) play a rôle between that of the minimal surfaces, on which the invariant conjugate system consists of two families of minimal lines, and the surfaces of Voss, on which the invariant conjugate system consists of two families of geodesics.

If the general solution θ of an equation of the form $\partial^2\theta/\partial u\partial v=M\theta$ is known, where M is an arbitrary function of u and v, and one can find three particular solutions, ξ, η, ζ, linearly independent of θ, the equations of a surface $S_0(x_0, y_0, z_0)$ referred to its asymptotic lines can be found by six quadratures from the formulas †

$$(13)\qquad \begin{cases}\dfrac{\partial x_0}{\partial u}=\zeta\dfrac{\partial\eta}{\partial u}-\eta\dfrac{\partial\zeta}{\partial u}, & \dfrac{\partial x_0}{\partial v}=\eta\dfrac{\partial\zeta}{\partial v}-\zeta\dfrac{\partial\eta}{\partial v},\\ \dfrac{\partial y_0}{\partial u}=\xi\dfrac{\partial\zeta}{\partial u}-\zeta\dfrac{\partial\xi}{\partial u}, & \dfrac{\partial y_0}{\partial v}=\zeta\dfrac{\partial\xi}{\partial v}-\xi\dfrac{\partial\zeta}{\partial v},\\ \dfrac{\partial z_0}{\partial u}=\eta\dfrac{\partial\xi}{\partial u}-\xi\dfrac{\partial\eta}{\partial u}, & \dfrac{\partial z_0}{\partial v}=\xi\dfrac{\partial\eta}{\partial v}-\eta\dfrac{\partial\xi}{\partial v},\end{cases}$$

* Bianchi-Lukat, *l. c.*, pp. 127, 135.
† Bianchi Lukat, *l. c.*, p 132 ff.

where $K = -1/\rho^2$ is the curvature of S_0 and $\xi = \sqrt{\rho}X$, $\eta = \sqrt{\rho}Y$, $\zeta = \sqrt{\rho}Z$. If ξ, η, ζ are chosen such that $\xi^2 + \eta^2 + \zeta^2 = [\phi(u) + \chi(v)]^2$, the associate of such a surface will admit of continuous deformation with preservation of conjugate lines.

As an example, consider the equation $\partial^2\theta/\partial u\partial v = 0$, of which the general solution is $\theta = U + V$. As three particular solutions, assume $\xi = v$, $\eta = \psi(v)$, $\zeta = u$, where $\psi(v)$ is arbitrary. Then $\xi^2 + \eta^2 + \zeta^2 = u^2 + v^2 + \psi^2$. Formulas (13) give for S_0 the right conoid,

$$x_0 = u\psi, \quad y_0 = uv, \quad z_0 = \int(\psi - v\psi')dv. \tag{14}$$

Then for the group of associated surfaces we have from (9),

$$x = \frac{\psi'(uU' - U) + \psi V' - \psi' V}{\psi - v\psi'}, \tag{15}$$

$$y = \frac{uU' - U + vV' - V}{\psi - v\psi'}, \quad z = U',$$

where the primes denote partial derivatives. These surfaces were first investigated by Mlodzieowski.*

For the fundamental magnitudes we find

$$E = \frac{U''^2}{(\psi - v\psi')^2}[u(1 + \psi'^2) + (\psi - v\psi')^2],$$

$$F = \frac{(v + \psi\psi')uU''}{(\psi - v\psi')^3}[V''(\psi - v\psi') + \psi''(uU' - U + vV' - V)],$$

$$G = \frac{\psi^2 + v^2}{(\psi - v\psi')^4}[V''(\psi - v\psi') + \psi''(uU' - U + vV' - V)],$$

$$D = \frac{U''}{\sqrt{u^2 + v^2 + \psi^2}}, \qquad D' = 0,$$

$$D'' = \frac{V''(\psi - v\psi') + \psi''(uU' - U + vV' - V)}{(\psi - v\psi')\sqrt{u^2 + v^2 + \psi^2}}.$$

* See Goursat. *Amer. Jour. of Math.*, vol. 14, pp. 1–8.

If in (14) we choose $\psi = 1$ we have the paraboloid, and from (15) as its associated surfaces

$$x = V', \qquad y = uU' - U + vV' - V, \qquad z = U',$$

which are the equations of surfaces of translation whose generating lines are plane and lie in perpendicular planes.

Putting $\psi = \sqrt{1 - v^2}$ and changing v to $\sin v'$, we have from (14), omitting the primes, the helicoid

$$x_0 = u \cos v, \quad y_0 = u \sin v, \quad z_0 = v$$

and, for its associated surfaces, the moulure surfaces

$$x = -(uU' - U) - V' \cos v + V \sin v,$$
$$y = uU' - U - V' \sin v - V \cos v,$$
$$z = U'.$$

Finally, putting $U = c\sqrt{1 + u^2}$, $V = 0$, $\psi = -1/b\sqrt{c^2 - a^2v^2}$, where a, b, c, are constants, and changing u to $\cot u'$, we have from (14), omitting the primes,

$$x_0 = -c/b \cot u \cos v, \quad y_0 = c/a \cot u \sin v, \quad z_0 = -c^2v/ab$$

and for the associated surfaces, the quadrics

$$x = a \sin u \sin v, \quad y = b \sin u \cos v, \quad z = c \cos u.$$

If S is any surface which admits of continuous deformation with preservation of a conjugate system, the different forms which S assumes during the deformation may be regarded as distinct surfaces, so that the deformation leads to a continuous system of applicable surfaces. To find the individual surfaces of such a system, let E, F, G, D, 0, D'' be the first and second fundamental magnitudes of S referred to a conjugate system of lines: then the corresponding magnitudes of any other surface of the system are E, F, G, λD, 0, D''/λ, where λ satisfies the equation *

$$(16) \qquad \frac{\partial \lambda}{\partial u} = \left\{ {1 \atop 2} {2 \atop 2} \right\}' (\lambda - \lambda^3), \quad \frac{\partial \lambda}{\partial v} = \left\{ {1 \atop 1} {2 \atop 1} \right\}' \left(\frac{1}{\lambda} - \lambda \right).$$

* Bianchi-Lukat, l. c., p. 336.

If the surface S_0 to which S is associated is referred to asymptotic lines, S and S_0 must have the same spherical representation and the symbols $\{^{1\,2}_{1}\}'$ and $\{^{1\,2}_{2}\}'$ will be the same functions of u, v whether calculated from the equations of S or from those of S_0. Using S_0, Codazzi's equations may be written in the form

$$(17) \qquad \{^{1\,2}_{1}\}' = -\tfrac{1}{2}\frac{\partial}{\partial u}(\log \rho), \quad \{^{1\,2}_{2}\}' = -\tfrac{1}{2}\frac{\partial}{\partial v}(\log \rho),$$

where

$$\rho = \frac{1}{\sqrt{-K_0}} = \phi(u) + \chi(v).$$

From (17) we have for (16),

$$\frac{\partial}{\partial u}\left[\log\left(\frac{\lambda}{1-\lambda^2}\right)\right] = -\frac{\partial}{\partial u}(\log \rho),$$

$$\frac{\partial}{\partial v}[\log(1-\lambda^2)] = \frac{\partial}{\partial v}(\log \rho).$$

Solving these equations for λ we find

$$\lambda = \sqrt{\frac{c+\chi(v)}{c-\phi(u)}},$$

where c is a constant. Then by Bonnet's theorem, to every value of c there corresponds a surface whose fundamental magnitudes are $E, F, G, \lambda D, 0, D''/\lambda$ and the determination of whose cartesian coordinates depends upon the integration of an equation of Riccati. In the case of the surfaces defined by (16) we have,

$$\lambda = \sqrt{\frac{c+v^2+\psi^2}{c-u^2}}.$$

PURDUE UNIVERSITY,
November, 1905.

THE NEW CALCULUS OF VARIATIONS.

THE title above will indicate the intention of the writer to allow himself more freedom of comment in the present paper than is usual in a review. In attempting to form a just estimate of Kneser's Lehrbuch* and in perusing Bolza's Lectures † as well as in the preparation of another paper ‡ dealing with the calculus of variations, the several books and memoirs which I shall mention have necessarily come to my attention, even though some of them have not been used extensively. It appears to me that a short paper which shall give an idea of the various books now before the public would serve two purposes: the valuable one of giving a comparative view of all, and the convenient one of condensing into one a number of separate reviews which might eventually weary the reader.

Of the old calculus of variations the mathematical public knows well. A book which will undoubtedly stand for all time as the last exposition of that theory in what was its best form is Pascal's Calcolo delle variazioni,§ which was published in German translation by Teubner in 1899.‖ That this translation — which is the edition to which we shall constantly refer — was thought worthy of publication and was actually the best extant treatise in 1899 is a curious commentary upon the suddenness with which the modern theory leaped into the public arena and upon the secrecy in which the previous developments of that theory had been veiled, especially when we note that the very next year is the date of Kneser's now famous Lehrbuch. To be sure Zermelo had in his thesis given the essence of the Weierstrass theory, and the papers by Zermelo and Kneser seem to have been familiar at least to the translator (see page 65 and footnote, page 65). But if evidence were needed that Weierstrass's theory was not generally known or that the papers mentioned had failed to make a noticeable impression upon the general mathematical public, one need not go beyond the present book in search of it, for the influence of the Weier-

* Lehrbuch der Variationsrechnung, by A. Kneser, Braunschweig, Vieweg, 1900, 8vo, 306 pp.

† Lectures on the Calculus of Variations, by O. Bolza. See BULLETIN, vol. 12, No. 2 (November, 1905) pp. 80–90.

‡ Article on the Calculus of Variations, Encyclopedia Americana, 1905.

§ Calcolo delle variazioni, by E. Pascal, Milan, Hoepli, 1897.

‖ Variationsrechnung, by E. Pascal, translated into German by A. Schepp, Leipzig, Teubner, 1899, 8vo, 146 pp.

strass theory upon this work is at most meagre. Pascal intended to make his book largely historical and he succeeded. This success now saves it from the oblivion which is the fate of many another book of scientifically the same class. The fourteen pages of historical matter at the beginning give only a partial idea of the amount of useful information of that nature in the book, for every chapter contains thorough references and some of the chapters are little else than abstracts of the theory and historical comment.

I shall not enter upon a detailed discussion of the contents, since the problems treated and the methods used are generally well known. Suffice it to say that nearly every problem of the old theory is mentioned in its general form: the simplest problem, the extensions to cases where higher derivatives enter the integrand, where multiple integrals occur, where auxiliary conditions are imposed, and so on. Thus most of the formal results are presented here in compact and convenient form. But the critical spirit is not so marked as it must be henceforth. As a single example may be mentioned the absence of exact statements concerning the very definition of a minimizing curve. Indeed the author points out (pages 16 and 17) that "certain categories of curves" are arbitrarily excluded as comparison curves on account of the fact that the nature of the "variations" allowed restricts the comparison curve so that "all the derivatives approach the derivatives of like order for the supposed solution." But this remark goes no further than did the almost identical remark made by Legendre (see § 30, page 109). Surely Zermelo's dissertation should have been mentioned in this connection, especially in the discussion of Newton's problem (§ 30),* and the failure to take account of Zermelo's exposition of Weierstrass's theory results, both here and elsewhere, in the well-known misstatements common to all the older works.

Finally, one feature which remains valuable should be mentioned. Practically the whole of the last thirty pages — nearly one-fourth of the book — is devoted to special problems and applications, including most of the famous problems and such general theories as those of minimum surfaces, geodetic lines, etc. While the treatments given do suffer from the lack of precision noted above, it should be kept in mind that the

* Compare Kneser, Encyklopädie d. Math. Wiss., II A 8, p. 609.

solutions obtained must include the rigorous solutions at any rate, and that the curves which are declared to be minimizing curves would in general at least render the integral a "weak" minimum. In seeking a rigorous solution, therefore, these results are still by no means to be despised.

A very useful bibliography of the subject, which practically exhausts the literature up to 1890 at least, forms a fitting sequel to a book which Pascal has made curiously important from a historical standpoint.

In striking contrast to the spirit as well as to the contents of Pascal's book, there appeared in 1900 a new treatise by Kneser.* I have elsewhere † claimed that this book opened the doors of modern research in the subject to the general mathematical public, and its importance really merits a much more extended and probably a more favorable review than I shall be able to make at this time. The book would undoubtedly have been reviewed in these pages ere now, had the task seemed entirely easy and attractive to any one of several Americans who were otherwise interested in its appearance and its contents.

It is especially easy to draw broad comparisons between Kneser and Pascal. The latter's treatment is very clear, it gives a good general view of the subject as Pascal knew it; its theorems and other statements are well outlined and set in prominent types, it leans heavily toward historical comment; but it is lacking in rigor from the modern standpoint. Kneser's work, by contrast, is not lucid, its arrangement gives only a clouded view of the author's own conceptions and of the subject itself, the theorems and other similar statements are hidden amid a mass of discussion as if with conscious and consummate cunning, it shows an apparent tendency to conceal historical development, but it is a vast advance over any former work in its exactness.

Kneser published at the same time an article in the Encyklopädie,‡ which has been sent out only this year, but which was written and set up along with the Lehrbuch, and which should be considered at the same time. I shall refer to this second treatment as Kneser's *article*.

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*See first footnote, p. 172.

†See BULLETIN, loc cit.

‡ Encyklopädie der Math. Wiss., II A 8., pp. 571-625. Article entitled Variationsrechnung.

In considering these two treatments by Kneser it should be noted that both of them formally appear in print under the date 1900, though the article has not been given to the public until very recently. It follows that we shall not expect to find in either of them any of that fundamental work which Hilbert has been doing since 1900. It will be very useful in this connection to refer to Bolza's Lectures on the calculus of variations, which has been reviewed in the BULLETIN,* for Bolza in his chapters numbered V and VI follows Kneser rather closely. On account of the greater clearness of style, and on account of the development of the theory in the interval between the two dates of publication, Bolza's presentation is an enlightening commentary upon Kneser's work — an almost indispensable aid in deciphering much that is obscure in Kneser's own point of view. Surely a review of Kneser's Lehrbuch is immensely simplified through the existence of Bolza's book.

The first striking characteristic of Kneser's Lehrbuch is his exclusive use of the parameter representation of Weierstrass. The standpoint assumed by Bolza in his Lectures † will appeal to most teachers on account of its pedagogical correctness. Thus Bolza apparently appreciates the parameter representation quite as fully as does Kneser, but he sees and utilizes the possibility of giving the main essentials of the theory in the simpler asymmetric form first, and then passing to the more involved parameter representation later, by means of generalizations which are often trivially simple when the main facts are already established. This advantage in style Kneser missed: in consequence his work suffers in clearness, though it might be possible to write a very readable book based on the parameter representation alone. Aside from this, however, I am inclined to believe with Bolza (Lectures, page 115) that it is unjustifiable to discard the asymmetrical forms entirely in favor of the parameter representation.

Let us pass in review briefly the various chapters. The first chapter contains a statement of the problem in parameter form and the formal transformation of the first variation. The theorems of Weierstrass concerning integrals which are independent of the choice of parameter, and the corresponding methods of transforming the first variation were previously known only through the meagre publications of Weierstrass's students, and

* Vol. 12. No. 2 (November, 1905), pp. 80–90.

† See second footnote, p. 172.

form a necessary and important part of the introduction. The assumption on page 10 that the second derivatives exist for the solution is now known to be unnecessary, through the DuBois-Reymond-Hilbert proof, but such an assumption does not vitally injure the work and might even be defended as justifiable by one who was conscious of its superfluity.* The non-parameter form is explained in § 5 as a special or dependent case. Kneser's article in the Encyklopädie shows a striking contrast to the treatment of the Lehrbuch in that the parameter representation is mentioned in the article only in the briefest possible manner. The difference may be due to the great limitation which the author imposes upon himself in the use of work due to Weierstrass in the article (see Encyklopädie, II, A, 8, page 608, footnote 121*a*). In the Lehrbuch, though Kneser limits himself to work actually published by others, he does at least permit himself enough freedom to use Weierstrass's essential methods as presented by Zermelo and others. Again, the article is historical to a large extent, so that Weierstrass's results consume relatively little space, and the impossibility of giving an account of earlier investigations in parameter form may well account for the extreme difference between the Lehrbuch and the article.

The second chapter of the Lehrbuch (pages 10–43) is devoted to the derivation of necessary conditions. The proof of Euler's (or Lagrange's) condition follows easily from the transformed first variation by means of DuBois-Reymond's lemma (§8). In this connection, Kneser dwells upon the now familiar idea, which is due to Kneser himself, of the extremals, i. e., the solutions of Euler's equation. After giving a list of examples, Kneser proceeds in § 10 to what is perhaps the key note of his whole theory :† the consideration of integrals in which the end points are variable. As a matter of fact, these problems lead naturally to a generalization of the theorems concerning geodetic lines on a surface, especially after the introduction of the important idea of transversal curves. The whole problem of variable end points may also be thought of as an analogon of the boundary value problems of the theory of differential equations ; it is perhaps even more interesting from this point of view than from the analogy to geodetics, but both points of view should be kept in mind and both might

* Compare, for example, the results in § 17, p. 58.
† See Bolza, Lectures, Chapter V.

have been emphasized even more than Kneser has seen fit to do, without overstepping the proper bounds of the calculus of variations. A brief consideration of the isoperimetric problem closes the chapter.

One expects to find next a treatment of the so-called "second variation," according to the methods of Jacobi and his school. This expectation is based wholly on tradition and on the stress which Kneser lays in his article on the same methods. Here, however, Kneser leaves the beaten paths absolutely and begins at once a consideration of the sufficient conditions. The ideas developed are new and important in almost every instance. The idea of a field of extremals (§14) is the first and most fundamental. The following article on the extension of Gauss's theorems concerning geodetics is scarcely less important. Then follow the essential methods according to Kneser's theory for the proof of the sufficient conditions (§§ 16, 17), including the introduction of Kneser's curvilinear coordinates based upon a set of extremals and a corresponding set of transversals, and the essential distinction between strong and weak minima. These articles lead up to sufficient conditions, which Kneser sums up on page 60 under the titles "Jacobi's condition" and "Legendre's condition." The former is essentially the same condition as that elsewhere given as Jacobi's condition. The condition called "Legendre's" is that known to Legendre only in the case of weak minima. In the case of strong minima the condition is radically different, since it practically involves "Weierstrass's" condition, and might well be called "Weierstrass's" condition rather than "Legendre's." I shall not discuss in detail the remainder of this chapter, though it is important throughout. Among the topics considered are the methods of Weierstrass, including the E function; the envelope of a field of extremals; the Jacobi-Hamilton theorems. An amount of this matter will be found either reproduced or independently collected in Bolza's Lectures. A few errors or oversights in this chapter have been noted by Bolza in his Lectures and by Zermelo and Hahn in the Encyklopädie. Thus Kneser's proof of the existence of a field (§ 14) must be supplemented by a theorem due to Osgood (see Bolza, Lectures, page 176); and the statements of Jacobi's condition require certain revision (see Encyklopädie II, pages 630, 632), which have been given by Osgood, Bolza, and Bliss in memoirs quoted by Zermelo and

Hahn. The latter revisions have been conducted by means of considerations of the so-called "second variation," which would seem to indicate a present necessity for retaining the older considerations of Jacobi, at least until the proofs in question have been made upon the basis of the newer theories.

Kneser's fourth chapter treats the isoperimetric problem both for fixed and for variable end points. The principal methods and results of the chapter are presented in somewhat simpler form by Bolza (Lectures, Chapter VI). While the theorems are frequently essentially the same as those previously stated, some of the results and some of the methods employed are new. Any future work on this subject will necessarily use many of the ideas which Kneser introduces in this chapter.

The last four chapters are devoted to discontinuous solutions, the problem in which higher derivatives enter in the integrand, the most general problem of the calculus of variations, and double integrals, respectively. These chapters are certainly important, and they make the work complete in a sense in which it could not be without them. The previous chapters give the character to the book, however, and they are the especially interesting and important portions. Such an explanation is necessary in passing them over with no detailed mention, in order to avoid a misconception on the part of the reader. The high plane upon which Kneser has placed his work in the earlier chapters is fully maintained to the end.

In general, Kneser's Lehrbuch must surely be assigned a very high scientific value, and its many contributions will surely remain essential for the further development of the subject for many years. It is only from the pedagogic side that essential criticism can justly be made. The work is of monumental importance; it might have been of truly immense influence upon all classes of students of mathematics, had it not been for the unfortunate style of the author and for the resulting unnecessary complication and intricacy of statement and proof which characterize it from the standpoint of presentation. A previous remark of mine * to the same effect has been misunderstood in a curious manner by Professor Haussner (*Fortschritte der Mathematik*, volume 33, page 379), who himself repeats my own criticism and my own praise in slightly greater detail. For this reason I might now insist that a presentation can justly be criticized for its complication when and only when it is unneces-

* BULLETIN, vol. 9, No. 1, October, 1902, p. 24.

sarily intricate, i. e., in so far as the complication arises from the style of the writer rather than from the nature of the problem. And can there be any doubt that any intrinsic difficulty or intricacy in the subject matter itself calls for especial care upon the part of a writer who wishes adequately to present the subject or any portion of it?

Kneser's article in the Encyklopädie, which has been quoted above, is scarcely to be classed with his Lehrbuch. Its whole tone is rather historical than scientific. Its value is further limited by the attitude of the author toward Weierstrass's work, which, though well known through practically public lecture notes, is refused recognition here except when accidentally published by another author. The scruple against presenting the unpublished work of another man is honorable in general, but in this instance no possible harm could have been done, and the fame of Weierstrass would be better guarded by a frank reference to his unpublished work than by silence at a time when a large part of the modern developments in the calculus of variations can be traced directly or indirectly to his influence. The references given in footnotes supplement to a very considerable degree the lack of such references in the Lehrbuch; otherwise the Lehrbuch is a more systematic presentation of the subject and is, of course, much more detailed in its treatment than the article. It would therefore seem that the only present value of the article is its historical value, including its copious notes.

The article by Zermelo and Hahn,* which immediately follows Kneser's article in the Encyklopädie, is much more important at the present moment, since the work reviewed in it is not as yet fully reproduced in any treatise. To be sure a number of the results are given by Bolza in his Lectures, but there are very many points of interest upon which Bolza does not touch. This article covers the period from 1900 (when Kneser's article was finished) to 1904 (when both articles actually appeared). It is probable that no equal period of time has seen such an advancement in the subject as this, and it is certain that at no time have so many minds been devoted to its investigation. The article covers only fifteen pages and, therefore, it is necessarily only a summary of facts. The footnotes, however, con-

* Encyklopädie der Math. Wiss. II A 8*a*, article entitled "Weiterentwickelung der Variationsrechnung in den letzten Jahren," pp. 626–641.

tain complete references to the original sources. The recent work of the Hilbert school seems to predominate in the exposition in the constant recurrence of the important ideas of the Hilbert invariant integral and the Hilbert a priori existence proof. Of course many authors in no way allied with Hilbert are repeatedly mentioned. In particular the work of Weierstrass was an essential precursor of that of Hilbert, and Kneser's work is of great importance, independently of any of Hilbert's work. The next most prominent names seem to be those of Osgood, Bolza, and Bliss; these three Americans and to a lesser extent a few other Americans have not only made the modern calculus of variations familiar to the American public, they have advanced to a marked degree the general knowledge of the subject in the important papers referred to in this article. The topics are the Weierstrass theory, the simplest problem, the isoperimetric problem, the general problem, applications, and the existence proofs. In many particulars this article is at present the most authoritative and exhaustive presentation of the results of recent investigations in the subject.

The last book I shall mention is also of American origin.* It represents substantially the lectures delivered in regular courses on the calculus of variations by the author, Professor Harris Hancock, at the University of Cincinnati. Professor Hancock bases his work almost exclusively upon the lectures of Weierstrass and Schwarz which he heard at Berlin, and consciously disregards the developments of later years. Since the work of Weierstrass is already fairly well known in even more authoritative form through the well-known lecture notes at Berlin, the present book is shorn of much of the value it might otherwise have from a historical standpoint. An official publication of Weierstrass's work by those who now hold his original papers would possess extreme interest in showing precisely Weierstrass's point of view, whereas it is not always evident in the present book whether the statements and the proofs, in the precise form in which they are given, are due to Weierstrass or to Hancock. For this reason it is difficult and rather unnecessary to review the book in detail. It may be noted that a considerable portion of the contents has already been published by Professor Hancock in the *Annals of Mathematics* (first series, volumes 9–12).

* Lectures on the Calculus of Variations, by H. Hancock, Cincinnati, University Press, 1904, 8vo, 292 pp.

Beside the books and articles mentioned in this review, several of the recent treatises on the calculus contain a chapter on the subject. Among these should be mentioned at least the new edition of Serret-Bohlmann, Differential and Integralrechung, volume 3, and Goursat, Cours d'Analyse, volume 2. These books will doubtless be reviewed in their entirety in the BULLETIN, and I therefore satisfy myself here with a mere mention.

The works mentioned attest in the strongest possible manner the extraordinary vogue into which the calculus of variations has suddenly sprung. The cause is not far to seek: it is the revelation through the work of Weierstrass, Kneser, Hilbert and others that the calculus of variations is susceptible of the same exquisite rigor which had previously existed only in the theory of functions of a real variable, and that a wide field of research and rich discovery was opened by such methods.

Although the end of these investigations has by no means been reached in this single subject, it is not premature to suggest the analogous development of other mathematical theories along equally rigorous lines, and also the construction of a supplementary theory in each of them which shall be as rigorously applicable to general geometric problems as is the Weierstrass theory in the calculus of variations. E. R. HEDRICK.

COLUMBIA, MO.,
November, 1905.

GRANVILLE'S DIFFERENTIAL AND INTEGRAL CALCULUS.

Elements of the Differential and Integral Calculus. By W. A. GRANVILLE, Ph.D., with the editorial cooperation of PERCY F. SMITH, Ph.D. Ginn & Co., 1904. 463 pp.

So many text-books have been written upon the elementary branches of college mathematics that a raison d'etre can properly be asked upon the appearance of each new work. The great number of American text-books upon such subjects as college algebra, trigonometry and calculus, duplicating one another in aim and character, is in striking contrast with the paucity of our text-books upon more advanced mathematical subjects. What, then, we naturally ask, is the purpose of this new treatise, and what does it seek to accomplish which has not been already accomplished?

In the introduction the authors tell us that "the present volume is the result of an effort to write a *modern* text-book on the calculus which shall be essentially a drill book." In making this quotation I have taken the liberty to italicize the word "modern," inasmuch as it seems to me that the chief interest of the book, and the first as well as the last questions concerning it center in this word. The need of modern text-books on calculus is recognized by every one who is familiar with its modern developments and reconstruction. The older English text-books — as, for example, those of Todhunter and Williamson — fail altogether to satisfy the recent demands for precision and rigor. Some of the most fundamental concepts have not been formulated with mathematical definiteness, and hence the consequences of concepts and definitions can not be clearly traced. As a result, the theorems are commonly stated without restrictions. But, as universals, they are rarely true. Thus while the student gains a good knowledge of rules and formulas, he is left in ignorance both of the limits of their applicability and of the restrictions which must be imposed upon the functions to bring them under the theorems. Text-books of this nature are only too likely to produce thinkers of the same indefinite character; for like breeds like. Recently two or three English text-books, in particular Lamb's and Gibson's, have been written to remedy this defect. But notwithstanding manifold excellences these books are scarcely serviceable for a first course in our American colleges.

The composition of our classes often gives rise to grave pedagogical difficulties in teaching the calculus. Very rarely do these classes consist of a few choice spirits, for whom calculus is a preparation for the higher mathematics. The bulk of the class usually consists of men of very varied aims and ability. Some there will almost certainly be who are pursuing calculus for the sake of its applications in physics, chemistry and other sciences, as for example the engineer, by whom calculus is often used as a mechanical tool; some there may be who are preparing to teach mathematics in secondary schools, who do not care to advance into its higher branches and yet wish to gain a glimpse of modern methods and conceptions; others perchance who finish their mathematical education with calculus and aim merely to review and strengthen their previous knowledge; others still, who, under our elective system, take the course by instinct, without any definite object, because they

have "a fondness for examples" and for working out the neat riddles and puzzles which mathematics affords. Such elements, and even others, may be included in a class. In such a constituency we find a "mathematisches Publicum" of the greatest interest and importance, from which we must continue to draw much of our best mathematical talent. It is, moreover, this Publicum which must be carefully cultivated to preserve for mathematics that wide influence on thought which it has had in the past, and which is so admirably traced in Merz's History of European thought in the nineteenth century.*

How shall calculus be taught to classes of such varied composition as has been described? How far is it possible, and, if possible, is it desirable and wise to introduce the modern developments into elementary teaching? The position assumed by the authors in their new treatise in answer to this question seems to the reviewer both a sane and a practical one.

First of all, it must be kept in mind that for an ordinary class the text-book must be primarily a drill book. Dr. Granville understands thoroughly this need, and it may be confidently predicted that his text-book will be a most decided success in the class room. Unusually attractive in appearance, the volume presents the subject matter clearly and bears the marks of a teacher who feels a student's difficulties. There is an unusually large and well-graded stock of examples, from which the student will gain an admirable review in the branches of mathematics preceding calculus. In this particular Granville's book resembles Osborne's treatise. It is provided also with good collections of formulas, of integrals, and of curves, for reference.

How to write such a drill book and keep it in touch with modern analysis is the chief problem of the author. This accord has been sought by drawing upon intuition for certain principles, of which the proof is postponed to a second course in the calculus. Upon these principles as a firm foundation the calculus can then be securely built. Such, at least, is my interpretation of what has been and should be sought.

Consider first the application of this method to the differential calculus. Here the foundations for a rigorous treatment

* This book is pervaded with a mathematical spirit. The influence of mathematics upon thought is considered, beginning with the seventeenth century.

are Rolle's theorem and the theorems of mean value. These are easily approached from the intuitional side. No attempt is made by the author to reproduce the delicate analysis by which they are established. He clearly recognizes that such an analysis is not suited for an elementary course. But his very brief intuitional and geometric treatment will be grasped at once by the student. Once obtained, these theorems serve as a sound basis for the theory of indeterminate forms, for Taylor's formula * and series, for the theory of envelopes, etc. This plan on the one hand has the advantage of avoiding considerations which are premature, and on the other hand removes the necessity of unlearning subsequently false demonstrations and methods. No such fallacious proofs need be foisted upon the learner as are found in many text-books for the expansion of a function into Taylor's series or for the theorem that in the partial differentiation of a function of two variables the order of the differentiations is immaterial. Such proofs confuse and enfeeble the perceptive powers of the learner.

Complete success in infusing an elementary text-book with the modern spirit could scarcely be expected. Let me point out one or two places where in pouring the new wine into the old bottles the skin has been badly cracked. One of these rents is found in the very theorem which was mentioned last; namely, that in obtaining $\partial^2 f(x,y)/\partial x \partial y$ the order of the two differentiations may be reversed. Here, in applying the mean value theorem to the two increments

$$f(x+\Delta x, y) - f(x, y) = \Delta x \cdot f_x(x + \theta_1 \Delta x, y) \qquad (0 < \theta < 1),$$

$$f(x+\Delta x, y+\Delta y) - f(x, y+\Delta y) = \Delta x \cdot f_x(x + \theta_1 \Delta x, y + \Delta y),$$

the fatal slip is made that a common value θ_1 is taken for θ in the two equations. (Cf. equation C, page 206.) The oversight contained in the proof (§ 124) of the rule for evaluating the indeterminate form ∞/∞ is certainly surprising.†

Let us turn now to the integral calculus. Here the first pedagogical difficulty relates to the definite integral

$$\int_a^b \phi(x)dx,$$

* Called by Granville "the extended theorem of mean value" (§ 118).

† When h approaches 0, $f(b)$ approaches ∞ so that $f(b)/F(x)$ itself tends to take the form ∞/∞ !

defined of course as the limit of

$$\sum_{x=a}^{x=b}\phi(x)dx.$$

Shall the existence of Darboux's upper and lower integrals be first demonstrated, and afterward their equality for a continuous function? Or shall the scope of the concept of an integral be contracted by taking all the increments dx equal to each other and multiplying each increment by the value of $\phi(x)$ at the beginning of the increments, as is done in most of our text-books? Granville and Smith wisely seek a middle course. Neither Darboux's integrals are introduced nor the above restricted concept of an integral. Appeal is again made to geometric intuition. By the consideration of the area under the arc of a continuous curve it is made clear that the two restrictions above described are unnecessary, and a corresponding analytic investigation is added, though of a somewhat incomplete character. I say "incomplete" because the investigation proves only that if $\phi(x)$ is continuous between $x = a$ and $x = b$, and *if there is a corresponding function* $f(x)$, *of which* $\phi(x)$ *is the derivative*, then there must be a limit for

$$\sum_{x=a}^{x=b}\phi(x)dx.$$

This is shown by the aid of the mean value theorem. Evidently the authors have thought it wisest to content themselves with this result. But in the opinion of the reviewer the all important fact should be blazed before the student that every function, continuous between a and b inclusive, has an integral between these limits. This theorem is not even stated, although obviously implied in the concept of the area under a curve. Moreover, it is needed by the author himself to complete his derivation (page 379) of the formula

$$s = \int_a^b \sqrt{1 + (f'(x))^2}\, dx$$

for the length of a curve.* A suitable and simple proof of

*The theorem could have been used also as the basis of other formulas of the integral calculus, and is then preceded by a summation. This is the integral method of proof. The author chooses the differential method. Cf. §§ 223, 227, 228.

the theorem could be given readily, without entering upon all of the work necessary to establish Darboux's upper and lower integrals, with the aid of the geometric intuition that when $y = \phi(x)$ is continuous in an interval, a value δ can be found so small that the variation of $\phi(x)$ in any subinterval less than δ in length will be less than any arbitrarily assigned positive magnitude.* This very fundamental intuition is almost hidden by the author in the fine print of page 370; it should be thrown into bold relief.

A second deficiency is the failure to exhibit the two definitions of the definite integral — obtained (1) from the limit of a sum and (2) from the difference of two values of the indefinite integral — as two wholly independent and by no means coextensive definitions.

In these and some other particulars it is to be hoped that when a second edition of the book appears, it will be permeated yet further with the modern spirit. The restrictions respecting continuity imposed upon the derivatives in various theorems might be incorporated advantageously into the theorems themselves, instead of being relegated to footnotes or altogether omitted (e. g., § 118). Continued insistence that the student shall trace the effect of so primitive a concept as continuity and shall note the conditions introduced successively into a theorem or into a chain of theorems will help to impress upon him the fact, too often overlooked, that careful observance of the limitations imposed by the reasoning is fundamental in algebraic analysis as well as in synthetic geometry.

I have dwelt, perhaps unduly, upon certain aspects of an admirable text-book because of my great interest in the adaptation of calculus to the beginner and because of a belief that the most practicable plan is that followed by the authors, to use intuition when and only when it has been justified by exact analysis and then to build boldly upon it.

* For first, it could be shown immediately from this intuition that the difference between the values of

$$\sum_{x=a}^{x=b} \phi(x)dx$$

for any two partitions whatsoever of the interval (ab) can be made as small as we choose by taking the maximum of $|dx|$ sufficiently small. Then it would remain only to prove that for some system of partitions $\Sigma\phi(x)dx$ converges to a limit. The latter fact is evident since for any infinity of values of $\Sigma\phi(x)dx$ there must be at least one point of condensation.

A few words should be added about the contents of the book. The usual range of topics is included, without neglecting applications in mechanics and with the addition of brief but suitable chapters upon ordinary differential equations and upon tangent lines and planes in space. Special attention is given to the parametric representation of curves. An unfortunate omission will be noted under the topic of differentials. It is not proved nor even remarked that the differential and increment of a function $f(x)$ differ from each other by an infinitesimal of higher order, although the proof of this important fact would occupy only a few lines. The application of differentials to the approximate computation of small increments of $f(x)$ is simultaneously excluded. The corresponding omissions in the case of the differential of a function of two or more variables are especially to be regretted, for the differential (§§ 136, 137) is left devoid of significance when the variables are independent.

In conclusion, generous recognition should be accorded to the care which has been bestowed upon the work. At many points improvements over our current text-books will be noticed, not in themselves sufficiently important to dilate upon but having together great cumulative force. As an instance, I shall cite the inclusion of a real proof that two functions which have a common derivative can differ only by a constant. The introductory chapter on the concepts continuity, function, and limits can also be especially commended, and the chapters on series and the expansion of functions. I know of no work which has greater promise of success in our college classes.

EDWARD B. VAN VLECK.

GENEVA,
September 20, 1905.

THE FOUNDATIONS OF SCIENCE.

Wissenschaft und Hypothese. Von HENRI POINCARÉ. Autorisierte deutsche Ausgabe mit erläuterenden Anmerkungen von F. und L. LINDEMANN. Leipzig, B. G. Teubner, 1904. xvi + 342 pp.

NOT logical enough for the logician, not mathematical enough for the mathematician, not physical enough for the physicist, not psychological enough for the psychologist, nor metaphysical enough for the metaphysician, Poincaré's Science and Hypothe-

sis can hardly give the satisfaction of finality to any one; and yet it probably comes nearer to satisfying the requirements of all these classes of investigators than any single book of our acquaintance. To the author this antithesis ought to be the source of the greatest gratification, in view of his position that a theory is never of greater service to science than when it breaks down. For this little book breaks down constantly, although it is full of brilliant suggestion on every topic. Were it possible to find from the pens of the leading logicians, psychologists, physicists, and metaphysicians such works as this from the pen of a supreme mathematician, there would be at hand the material for reaching a satisfactory and unified theory of science — provided, of course, that such a theory is possible. But is it possible? And even if, to follow Bôcher's happy phrasing,* some method of successive approximation is really leading us toward absolute rigor and unity, why should we assume that we shall ever reach the limit, or even that the limit exists?† May we not be forever forced to rest content with a mere feeling of convergence —slower or faster, and very likely asymptotic at best? If so, works like the present will always remain necessary and highly useful, perhaps more useful than those which give a more consistent theory which is less true to natural limitations.

In Part I the author analyzes the concepts of number and magnitude. He affirms (page 17) that the principle of mathematical induction and it alone can teach us something new. The argument is plausible but seems at times too narrow, founded too much on the principle of arithmetization in its most meager form, and too closely associated with the syllogism as the fundamental method of reasoning. In geometry, if not in algebra, one feels conscious of other methods of proof, other methods of passing from the particular to the general and conversely. Furthermore the symbolic logicians have pointed

* BULLETIN. vol. 11 (1904), p. 120.

† It should be remembered that in order to classify and otherwise deal with experience, science is bound to idealize. For different purposes different idealizations are convenient, and these may sometimes be mutually contradictory without introducing great inconvenience. Take, for instance, the conception of infinite. The idea of finiteness is definite. When, however, we negative the idea, we are often led to harmless contradictory statements. Thus "the portion of the plane which is at infinity is a line" and "the portion of the plane which is at infinity is a point" cannot both be true, but are in constant use as different idealizations, and introduce no difficulty when each is kept in its own field. Recently a great deal of disagreement has arisen in trying to relate the transfinite ordinals and cardinals — perhaps they are harmlessly contradictory.

out the fact that other modes of reasoning than the syllogistic are valid and essential parts of our logical machine. In the subsequent discussion of irrational numbers and the continuum, the author's point of view comes emphatically to the foreground, namely, that it is physics which is the real thing, that our mathematics is purely arbitrary and a matter of convenience alone, always guided however by our sense impressions and hence somewhat limited in its arbitrariness as far as practical results are concerned. He says: "The mind does not use its creative faculty except when experience forces it to do so" (page 30.) Although a pure mathematician, imbued to a less extent than is Poincaré with the idea that it is applied mathematics which is essentially important and that pure mathematics is only relatively so, might dissent from this opinion, there is contained in it at least a highly valuable pedagogic suggestion. The main difficulty in getting students interested in pure mathematics consists in overcoming their inability to see what it is all for.

Part II on space and its relations to geometry and experience is one of the most original and suggestive in the book. A few quotations will serve to exhibit the author's point of view, if not his argument: "The geometric axioms are neither a priori synthetic judgments nor experimental facts. They are merely conventions, or in other words only disguised definitions. Geometry is not an experimental science, though experience guides us in setting up the axioms. The question whether the euclidean geometry is the *true* geometry has no more sense than whether the metric system of measurement is the true system (pages 51, 73, 138)." Bôcher has recently discussed this point of view, and come to conclusions with which we are in substantial agreement.* Poincaré goes on to analyze with care the relations between the geometric space which we imagine on account of its convenience and the various spaces which our senses of sight, touch, and motion afford. The characteristics of homogeneity and isotropy, for instance, are not due to any individual one of these spaces, but arise from studying the laws under which our sense impressions succeed one another. Here the author's point of view is largely that of the psychologist. It may be well to note that the French mathematicians are not content with the purely logical development of geometry from a set of more or less artificial postulates. They insist that psychology

* Loc. cit., p. 124.

be taken into account. Picard writes: * "In these questions, the geometric logicians appear to pay too little attention to psychology, and to the information furnished us by uncivilized peoples." (He is referring primarily to the concept of number, to which the argument applies with certainly no greater force than to geometry.)

This question of how far psychology of individuals or of races should be heeded is very interesting and brings up the whole matter of what the axioms or postulates are for and in how far they actually characterize real space. The mathematician is wont to say that, if the system of postulates is complete, it characterizes completely the (mathematical or logical) space in question. By this he means that he has defined, whether by postulates or nominally, a set of elements connected by certain relations and capable of being put into one to one correspondence with the elements of space in such a way as to leave the relations invariant. (As the elements in question are generally merely the real elements, the correspondence may cease to be one to one if extended to the domain of imaginaries.) For him space is any one of these sets of elements, or if he uses the principle of abstraction it is the class of all such sets. Thus his postulates are both necessary and sufficient for the demonstration of all theorems in his space, and for him the complete story of his space is told. But for the psychologist and the metaphysician the complete story is not told in any so easy fashion. The psychologist is interested in tracing space concepts to their origin in sense experience, the metaphysician, in estimating the objective validity of these concepts. Whether the mathematician shall sometime be able to help them further than at present is difficult to say. Lie, as a geometer, distinguished between groups that were similar; and perhaps the logicians may some day get a sharper criterion than the one to one correspondence.

Force is the title of Part III, in which the subject of mechanics, in its widest significance, is treated. Poincaré avowedly passes over the difficulties of space, which he has just been treating, and of time, of which he had included no discussion in this book, and proceeds to difficulties essentially dynamical with the following characteristic preface (page 92): "Thus absolute space, absolute time, and even geometry are not conditions which impose themselves on mechanics; they are no more pre-

* BULLETIN, vol. 11 (1905), p. 405.

requisite to mechanics than the French language is logically prerequisite to the truths which are expressed in French. It would be possible to try to formulate the fundamental laws of mechanics in a language independent of all these conventions. Naturally the formulation of these laws would become more complicated, because all these conventions have been imagined precisely for the purpose of abbreviating and simplifying that formulation." It should seem, then, to use the author's guiding idea of convenience, that mechanics is rather a prerequisite to geometry than geometry to mechanics. Thus geometry does become in a certain large sense an experimental science, even if the experiments must be made upon objects which are not geometric but physical. (See the discussion by Bôcher.)

A little farther on in the text (page 100) is made a statement upon which the metaphysician may well meditate: "That a definition may be of value it is necessary that it shall teach us to *measure* force; this, moreover, is sufficient; it is by no means necessary that the definition teach us what force *as such* is, nor whether it be the cause or the effect of motion." Much misunderstanding and useless discussion between metaphysicians and scientists might be avoided if the former realized that what the latter needs to *know* about force is just what the careless baggage man needs to know about a trunk — its magnitude and its direction — and that what the real content of the thing is may better be left to the metaphysician himself, as a sometimes too officious customs officer, to examine. (The same remark probably holds equally well with regard to the insertion of psychological elements among our geometric axioms.) But who can resist the temptation to examine more critically than is absolutely necessary for the mere physicist? The author himself goes further and comes to the conclusion, similar to that for geometry, that the principles of mechanics are but conventions or disguised definitions, quite arbitrary except in so far as suggested by experience as the most convenient to adopt. It is during this discussion that the now famous formulation of the principle of the conservation of energy occurs, namely: "There is something which remains constant." And another statement: "The two propositions, 'the earth turns' and 'it is more convenient to suppose that the earth turns,' have one and the same meaning." And again, apropos of the law of acceleration: "If the acceleration of a visible body appears to depend on something else than the positions and velocities of other visible bodies and of the invisible molecules whose existence

we have previously been brought to admit, nothing prevents us from supposing that this something else is the position or velocity of other molecules whose presence we have hitherto not suspected; and thus the law may be kept."

It requires these rather extensive quotations to bring clearly to light the author's underlying philosophical principle of convenience.* One might almost call his philosophy dogmatic. For, after obtaining a principle of wide application and becoming thoroughly used to interpreting nature or classifying experience by its aid, the most convenient thing to do is to introduce new terms and keep the principle rather than to find a new principle. Now undoubtedly for the mathematician such a procedure is always possible. For the physicist, however, it seems at times very artificial; and while it may serve as a first rate pragmatical doctrine to a certain recent school of metaphysics, there is apparently a decided tinge of superficiality about it. When one considers as practically identical the statements that the earth turns and that it is more convenient to imagine that the earth turns, he takes a position which seems to be either practical agnosticism or pure idealism, according to the interpretation one adopts. Whether this position is interpretable in such a way as to lead to a metaphysics satisfactory from all sides, only time can decide. Certainly, however, it is a great service that Poincaré has done in pointing out so clearly the fact that convenience plays a large rôle, that our knowledge is, as it were, a viscous fluid containing in itself traces of all knowledge that has been, with the weaker and more remote in time ever becoming less effective in determining the present state. The author seems to feel confident that we are approaching a steady state.

The last part of the work deals with nature. Before entering upon this subject the author draws a useful distinction between a law and a principle (page 141): "A law expresses a relation between two real terms A and B; but it is not rigorously true. We introduce arbitrarily an intermediate term C which is more or less fictive and which by *definition* is that which has to A *exactly* the relation expressed by the law. Thus our law is decomposed into an absolute and rigorous principle expressing the relation of A to C and an approximate experimental law expressing the relation of C to B." He goes on to say that in entering upon the study of nature we enter the

*Compare Mach's well known principle of economy in thought.

domain of laws properly so called. From this it is evident that in studying mechanics it is principles which concern us, whereas in studying physics it is laws. It is difficult to see how any such finely drawn distinction can exist in view of the most recent developments of electrodynamic theory. It is becoming more and more convenient to consider electricity as fundamental and mass as derived therefrom. That Poincaré did not foresee this and take it into due account is no blot on his excellent little book; but it goes to show how careful one must be in asserting, for instance, that even the euclidean geometry will always remain the most convenient. Naturally euclidean geometry will always be taught, and so will newtonian mechanics; they will remain the first and simplest approximations to ultimate theories of space and moving matter. But when one is treating the fundamental question of the relation of hypothesis to science, it is not the first but the last available approximation which is of importance. The author would probably be the first to admit this. *

Although what precedes may go some of the way toward giving an impression of what the book contains, it cannot bring out the charm, frankness, and directness of the text. Fortunately one may take his choice of reading in the original French or in the excellent German translation. † If, however, the matter of language is of no very great importance to the reader, he should by all means choose the translation. For the translators have done more than give a faithful rendering of the text. They have written a preface which sets forth clearly the essential points in the argument, and have appended about a hundred pages of notes of great mathematical, historical, and bibliographical value to the reader. Finally they have added an exhaustive index. Why the French so persistently leave the indexes out of their books is hard to imagine. The practice is certainly a great inconvenience, especially in such a work as the present, where constant reference to what has gone before and to what is to follow is necessary for the proper appreciation of the different sections.

EDWIN BIDWELL WILSON.

YALE UNIVERSITY,
November, 1905

* In fact the preface to his most recent series of essays "La valeur de la science," Paris, 1905, contains the statement that the changes in science which have taken place in the last few years call for a revision of some of the ideas propounded in the book here under review.

† An English rendering, containing a preface by Larmor and published by the Walter Scott Publishing Company, is reviewed by Russell in *Mind*, July, 1905, pp. 412–417.

LA MÉCANIQUE STATISTIQUE.

Elementary Principles in Statistical Mechanics, Developed with especial Reference to the Rational Foundations of Thermodynamics. By J. WILLARD GIBBS. Yale Bicentennial Publications. New York, Scribner and Sons; London, Arnold; 1902. xviii + 207 pp.

PEUT-ÊTRE les lecteurs du BULLETIN ne s'étonneront-ils pas trop de l'époque tardive à laquelle ils liront le compte rendu des Elementary Principles. Non seulement — est-il besoin de le dire? — ce livre n'est point de ceux que l'on analyse hâtivement; mais, d'autre part, les questions qu'il traite ont été vivement agitées dans ces derniers temps; les idées défendues par Gibbs ont fait l'objet de nombreuses controverses; les raisonnements par lesquels il les a appuyées ont, eux aussi, subi la critique. Il me paraît intéressant d'étudier son oeuvre à la lumière de ces controverses et en discutant ces critiques.

Gibbs craignait que sa Mécanique statistique ne semblât trop physique aux mathématiciens, trop mathématique aux physiciens. Nous ne ferons qu'énoncer un truisme en disant que ce scrupule exprime une des qualités précieuses de l'ouvrage. La nécessité d'associer, de la manière la plus intime possible, deux branches de la science qui s'étaient trop longtemps perdues de vue, n'est plus à démontrer, et il faut espérer qu'elle continuera à être comprise.

En fait, les physiciens ont déjà apprécié le livre de Gibbs et l'ont utilisé. On ne saurait mettre en doute son importance au point de vue des mathématiciens. Ceux-ci connaissent encore insuffisamment les théories cinétiques, où il semble que leur activité pourrait si utilement s'exercer. Non seulement ils ont besoin d'y être initiés; mais il ne leur est pas indifférent d'y être introduits par un des leurs, d'y trouver un guide qui d'abord s'appelle Gibbs et, ensuite, leur parle leur propre langue: on sait qu'à cette fusion des différentes branches de la science dont nous rappellions tout à l'heure l'importance, la discordance des points de vue auxquels elles se placent et la différence des éducations qu'elles supposent est un obstacle souvent plus sérieux que la multiplicité des connaissances qu'elles embrassent.

La "Mécanique statistique" est, en somme, une question

purement mathématique. Elle n'est autre chose que l'application du calcul des probabilités à la mécanique, que la dynamique d'un grand nombre de systèmes régis (dans le cas le plus simple) par les mêmes équations, mais avec des circonstances initiales différentes. Ce qui est observable et, par conséquent, important, c'est la valeur moyenne de telle ou telle quantité, fonction de l'état du système. A cette recherche, on substitue, avec Maxwell, celle du nombre de systèmes pour lesquels la quantité ainsi envisagée est (à un instant donné quelconque) comprise entre des limites données. Ce nombre sera, en général, très grand,—si grand que par un raisonnement logiquement absurde, fécond et rarement trompeur en fait, (du moins lorsqu'on n'entend pas le poursuivre jusque dans ses dernières conséquences) on applique la loi des grands nombres, même lorsque les limites dont il s'agit sont infiniment rapprochées.

Que seront, individuellement, les systèmes S auxquels s'appliquerons ces considérations? Gibbs, à l'exemple de plusieurs de ses prédécesseurs, fait abstraction de toute supposition particulière à leur égard: il part des équations canoniques d'Hamilton sous leur forme la plus générale, avec les coordonnées de position $q_1, q_2, \cdots, q_n$ et les "moments généralisés" $p_1, p_2, \cdots, p_n$ substitués aux vitesses.

Il y a là autre chose qu'une généralisation toute théorique. D'une part on ne sait encore trop sous quelle forme on arrivera à se représenter ces éléments matériels ou électriques, dont les évolutions compliquées seront chargées de rendre compte des propriétés des corps naturels.

D'autre part, les principes de la mécanique, eux-mêmes, sont en train de se recodifier: voici que la masse n'est plus une constante, mais une fonction de la vitesse. Une seule chose semble devoir subsister. Les nouvelles équations pourront, comme les anciennes, être déduites du calcul des variations; la nouvelle mécanique admettra elle aussi, un *principe de moindre action*, que l'on s'est déjà occupé de formuler.*

Or cela suffit pour que les équations canoniques continuent à être valables.

Gibbs les considère d'une manière presque exclusive: il ne fait jouer qu'un rôle tout épisodique au cas particulier où il s'agit de points matériels isolés. Par là, son travail gagne sin-

* Voir entre autres, deux Notes récentes presentées à l'Académie des Sciences de Paris, l'une par MM. E. et Fr. Cosserat, l'autre par M. Langevin.

gulièrement en importance. Il gagne aussi en simplicité: j'entends cette vraie simplicité qui consiste à débarrasser la question de tous les éléments inutiles. Je ne crois pas qu'une autre forme d'exposition soit aussi propre à montrer aux mathématiciens ce qu'ils ont surtout intérêt à savoir : la voie qu'ils doivent suivre pour le progrès de cette théorie qui est de leur domaine et qui s'est fondée sans leur concours. A la lecture des Elementary principles, cette voie se dégage clairement : c'est le développement des principes que la théorie générale des équations différentielles et la mécanique analytique doivent à M. Poincaré.

Tout d'abord, la notion *d'invariant intégral*, qui, dans les Méthodes nouvelles de la Mécanique céleste, a reçu les belles applications que l'on sait, a été mise dès l'abord à la base de la mécanique statistique. Les trois premiers chapitres de l'ouvrage qui nous occupe sont un commentaire de cette double proposition :

1° L'unité est un multiplicateur des équations canoniques :

2° On connaît donc un invariant intégral de ces équations.

Cet invariant intégral est le *volume* dans la terminologie de M. Poincaré. Gibbs lui donne le nom *d'extension en phase*. Une *phase* est un état du système S, caractérisé par sa position et ses vitesses, autrement dit, par les valeurs de $q_1, q_2, \cdots, q_n ; p_1, \cdots, p_n$ et que l'on peut, par conséquent, représenter par un point dans l'espace E_{2n} à $2n$ dimensions. Il arrivera d'ailleurs, en général, que ce point ne pourra pas occuper une position absolument quelconque dans l'espace E_{2n}, mais devra se trouver dans une certaine région limitée R_0 de cet espace. L'extension en phase est relative à une portion quelconque R de R_0 : c'est l'intégrale

$$\int\int \cdots \int_R dq_1 \cdots dq_n \, dp_1 \cdots dp_n.$$

Si l'on donne la position du point représentatif au temps t, on détermine implicitement, par cela même sa position à un autre instant quelconque t'. Si on prend, pour le premier point, successivement chacun de ceux de la région R, le second point décrira une nouvelle région R'. Celle-ci a même extension en phase que la première : l'extension en phase est invariante dans le temps (sur une même série de trajectoires).

Mais Gibbs attire l'attention sur une autre propriété que possède ce même symbole et qui, moins remarquable sans doute que la première, n'en est pas moins des plus importantes. L'extension en phase est aussi invariante par rapport au système de coordonnées employé. Quelque soit le choix des q_i, du moment que les p_i sont choisis en conséquence, les extensions en phase seront conservées. On peut donc parler de *régions dynamiquement équivalentes** de l'espace à $2n$ dimensions — ce qui n'aurait pas de sens absolu sans la propriété d'invariance dont nous parlons (ce qui n'aurait aucun sens, par exemple, si les équations n'étaient pas canoniques).

On peut aller plus loin : Gibbs opère sur les positions seules, ou sur les vitesses seules, comme il a opéré sur l'ensemble des deux. Il peut ainsi définir une *extension en vitesse* et une *extension en configuration.*

Si étroitement liées à l'extension en phase que soient les deux notions précédentes, c'est intentionnellement que Gibbs en ajourne l'étude à son sixième chapitre. Il doit d'abord aller au plus pressé, au but principal du livre et introduire les notions statistiques sur lesquelles il raisonnera.

Grâce à la convention qui permet d'appliquer la loi de grands nombres à un élément infinitésimal, on représentera le nombre des systèmes S contenus (à un instant donné) dans une extension en phase donnée (c'est-à-dire dont le point représentatif est dans une région donnée R de l'espace E_{2n}) par l'intégrale

$$\int\int \cdots \int_R D\, dq_1 \cdots dq_n dp_1 \cdots dp_n,$$

où D est une fonction des q et des p, *la densité en phase.* La connaissance de la valeur de D en fonction de $q_1, \cdots, q_n ; p_1, \cdots, p_n$ donne la distribution absolue des systèmes donnés entre leurs différentes phases. En général, la distribution relative, proportionnelle, intervient seule, et il suffit d'étudier, N étant le nombre total des systèmes S, le quotient

$$P = D/N$$

ou *coefficient de probabilité, la probabilité en phase* étant l'intégrale

* L'extension en phase est une notion de dynamique (et non de cinématique) quoique les forces n'y entrent pas, parce qu'elle dépend des masses.

$$\int\int \cdots \int P dq_1 \cdots dq_n \, dp_1 \cdots dp_n.$$

Enfin la quantité

$$\eta = \log P$$

est *l'indice de probabilité.*

L'équilibre moyen d'un ensemble de systèmes S tel que celui que nous considérons — ou, plus généralement, le régime permanent — correspond à un *équilibre statistique*, caractérisé par ce fait que la distribution en phase est indépendante du temps.

Toutes les quantités que nous avons définies plus haut, probabilité en phase, coefficient de probabilité, indice de probabilité, qui sont invariantes dans le temps sur chaque trajectoire, sont des intégrales des équations différentielles. La condition d'équilibre statistique est que ces intégrales ne contiennent pas t.

Gibbs considère, en particulier, la distribution dans laquelle l'indice de probabilité est une fonction, et même une fonction linéaire, de l'intégrale la plus simple, l'énergie : c'est la *distribution canonique*, qu'il introduit ainsi à priori (et qui d'après ce qui précède, reste telle dans tout système de coordonnées).

La probabilité en phase est-elle, en réalité, une fonction de l'énergie seule? C'est, on le sait, une des questions qui ont été le plus fréquemment discutées dans cette théorie. Gibbs montre comment elle se pose. Un exemple simple fait voir que, au moins à priori, des intégrales autres que l'énergie peuvent jouer le même rôle et régler les valeurs de la probabilité en phase. Il suffit de considérer des points matériels placés, soit dans une sphère donnée, soit (c'est le cas considéré par Gibbs) entre deux sphères concentriques, et rendus incapables de traverser la ou les sphères limites. Ce dernier résultat s'obtient pour Gibbs, en faisant intervenir des forces centrales attractives ou répulsives suffisamment intenses ; on peut aussi imaginer que ces sphères soient des parois solides le long desquelles les points considérés se réfléchissent (élastiquement). L'hypothèse de Gibbs a l'avantage de faire intervenir des vibrations qui offrent une certaine analogie avec la théorie des radiations. Mais, dans l'un ou l'autre cas, les moments cinétiques par rapport à trois axes rectangulaires issus du centre sont constants pour chaque point : rien n'empêchera de concevoir la probabilité en phase comme dépendant, non seulement de l'énergie, mais encore de ces trois moments ou, du moins — si l'on veut sauvegarder l'isotropie de l'ensemble — de la somme de leurs carrés.

Rien ne dit même que les choses ne se passent pas de façon plus compliquée encore. Tout ce que l'on peut dire en considérant, à priori, la probabilité en phase, comme une fonction régulière est qu'elle constitue une intégrale *uniforme* des équations de la dynamique. Mais *l'uniformité* n'a pas ici le même sens que dans les recherches de M. Poincaré, puis qu'il ne s'agit plus nécessairement de fonctions analytiques. Elle serait encore, cependant, de nature à restreindre d'une manière notable la catégorie des expressions acceptables pour P.

Quant au mode de continuité de cette fonction, nous ne possédons qu'une proposition capable — autant qu'on en peut juger actuellement — de nous éclairer à ce sujet: c'est celle que M. Poincaré a déduite de la notion d'invariant intégral. D'après ce théorème, que Gibbs énonce à une autre occasion (au Ch. XII), les solutions des équations de la dynamique possèdent, en général, *la stabilité à la Poisson*, c'est-à-dire que le point représentatif M repasse, en général, une infinité de fois dans le voisinage (aussi immédiat qu'on le veut) de toute position M_0 déjà occupée. Les trajectoires pour lesquelles cela n'a pas lieu sont *exceptionnelles:* leurs points de départ ne peuvent former une extension en phase finie, si petite soit-elle.

Si le point M pouvait s'approcher de M_0 dans toute direction tangente à la multiplicité $\epsilon = \text{const.}$ (ϵ désignant l'énergie), il n'y aurait point d'autre intégrale uniforme possédant des dérivées partielles que l'intégrale des forces vives. Si M pouvait, dans le cours du mouvement, passer infiniment près de tout point de cette même multiplicité, l'intégrale des forces vives serait la seule intégrale continue.

Ce n'est pas, bien entendu, ce qui à lieu dans l'exemple choisi tout-à-l'heure. Mais n'en est-il pas autrement dans des problèmes moins particuliers? Cette question, de pur calcul intégral, reste sans solution mathématique. Les physiciens sont obligés de la discuter comme bien d'autres — voire, de l'aborder expérimentalement, ainsi que l'a fait Lord Kelvin — sans attendre notre jugement sûr, mais par trop tardif.

Après avoir indiqué que la distribution canonique peut ne pas être la seule imaginable, Gibbs retourne à cette distribution canonique, pour y mettre en évidence les propriétés que possède le coefficient de ϵ dans l'expression linéaire

$$\eta = \frac{\psi - \epsilon}{\Theta}$$

(qui représente l'indice de probabilité η) ou plutôt l'inverse Θ de ce coefficient. *Ces propriétés sont tout analogues à celles de la température absolue.* Par exemple la quantité Θ se comporte comme la température dans le mélange de deux ensembles ; et, surtout, elle donne lieu, lorsqu'on la multiplie par $d\epsilon$ et qu'on tient compte, s'il y a lieu, des forces extérieures, à une équation toute semblable à celle que donne le principe de Carnot. Le rôle de l'entropie (changée de signe) est alors joué par la valeur moyenne $\bar{\eta}$ de l'indice de probabilité.

C'est seulement à ce moment que sont introduites les notions, mentionnées tout à l'heure, d'extension en configuration et d'extension en vitesse et comme conséquence, l'étude des valeurs moyennes (ainsi que des anomalies correspondantes) qui se rapportent non plus seulement à l'énergie totale, mais à l'énergie cinétique et à l'énergie potentielle, considérées isolément.

Les quantités Θ, $-\bar{\eta}$, que nous avons vues jouer respectivement, dans une distribution canonique, le rôle de la température et celui de l'entropie, sont-elles seules susceptibles de correspondre à ces deux notions? Et si la distribution cesse d'être canonique, sommes-nous sûrs que l'entropie sera encore (aux signes près) la valeur moyenne de l'indice de probabilité?

La réponse, au moins sous réserve de considérations nouvelles, est négative. On peut former d'autres expressions de formes différentes, coïncidant d'ailleurs avec les premières dans le cas de la distribution canonique et possédant, elles aussi, les deux propriétés auxquelles nous avons trouvé plus haut une analogie thermodynamique.

Pour arriver à ce résultat, Gibbs considère toutes les phases pour lesquelles l'énergie est inférieure à une certaine quantité ϵ. L'extension en phase V de l'ensemble des états ainsi définis est une certaine fonction de ϵ, dont la dérivée est désignée par e^{ϕ}.

Il est clair que les fonctions V et ϕ de l'énergie ϵ ont une importance toute particulière. Leur introduction dans l'étude d'un ensemble canonique revient à classer les systèmes S dont il se compose d'après leurs énergies, à le décomposer, autrement dit, en parties dont chacune contient les systèmes pour lesquels cette énergie est comprise entre des limites données. Si ces limites sont suffisamment rapprochées, on peut regarder, dans l'intervalle qu'elles déterminent, l'énergie comme constante et on a la distribution que Gibbs appelle *microcanonique*, sorte d'élément de la distribution canonique.

Ce sont des combinaisons diverses des fonctions V et ϕ qui dans le cas de la distribution microcanonique, puis même (par la considération de leurs valeurs moyennes) en dehors de ce cas, fournissent des quantités substituables à Θ et à $-\bar{\eta}$.

Il reste à traiter la question la plus importante et la plus délicate que soulève cette étude de la distribution en phase. Que devient cette distribution au cours du mouvement, lorsqu'on part d'un état quelconque: tend-elle, par exemple, à se rapprocher de la distribution canonique ou d'une distribution présentant des propriétés analogues?

C'est, en somme, la question vitale pour les théories cinétiques. Le paradoxe qui s'y rattache et qui semble, au premier abord, miner par avance toute théorie de cette nature est en effet le suivant. Comment, en partant d'équations de la dynamique, toutes *réversibles*, parviendra-t-on à des lois *irréversibles*, à la croissance de l'entropie?

Gibbs se demande donc—après avoir établi une série de propriétés de minimum qui doivent jouer un rôle essentiel dans cette étude — ce que devient le mouvement lorsqu'on le considère à de longs intervalles de temps. Il commence par indiquer, à cette occasion, le théorème de M. Poincaré sur la stabilité à la Poisson auquel nous faisions allusion un peu plus haut.

La contradiction apparente qu'il s'agit d'élucider est ensuite posée, sous une forme nouvelle et plus frappante encore que celle qui a été rappelée tout à l'heure. Cette entropie qui doit être croissante, les calculs précédents conduisent, comme le veut d'ailleurs la théorie de M. Boltzmann, à la rattacher à la valeur moyenne de l'indice de probabilité. Or nous savons que cet indice est constant dans le temps. Comment sa valeur moyenne pourra-t-elle varier de quelque manière que ce soit?

La comparaison employée par Gibbs montre avec évidence comment des singularités de cette nature s'introduisent nécessairement dans l'application des considérations statistiques à des systèmes dont les différentes parties se mélangent entre elles.

Soit un vase de forme cylindrique (dont, pour simplifier, on pourra considérer exclusivement une section droite) et supposons ce vase rempli de deux liquides A et B de telle manière qu'à l'origine des temps ces liquides soient distribués par secteurs: par exemple, deux diamètres perpendiculaires seront supposés diviser le cercle en quatre parties dont deux opposées contiendront exclusivement du liquide A et les deux autres du liquide B.

Imaginons maintenant que les différentes couches circulaires concentriques dont est composé le milieu soient animées chacune d'un mouvement de rotation, mais avec des vitesses angulaires différentes. Si nous suivons dans son mouvement une portion quelconque du liquide A, par exemple, son volume ne changera pas et sa densité restera également invariable. La connexion de ce volume ne sera même modifiée à aucun moment.

Cependant, une portion déterminée quelconque de l'espace intérieur au vase, si petite qu'elle soit et qui, à l'origine, renfermait exclusivement du liquide A ou du liquide B, renfermera pour peu qu'on suive le mouvement pendant un temps suffisamment prolongé un mélange des deux fluides, mélange sensiblement homogène* pour t suffisamment grand. En particulier la densité, tout invariante qu'elle soit, ne sera plus nulle part celle de A ou celle de B, mais aura pris une valeur intermédiaire.

Si l'on suit le mouvement à partir de l'origine, on verra cette densité s'égaliser ; l'homogénéité du liquide (et il est clair qu'on pourrait, de bien des manières, définir un nombre qui serve de mesure à cette homogénéité) ira d'une manière générale croissant.

Le parallélisme est, on le voit, complet avec le phénomène qui est en question pour la quantité $\bar{\eta}$, et, plus loin, Gibbs sera conduit à préciser et à montrer, comme Boltzmann, que cette quantité doit être décroissante.

Seulement, comme l'a remarqué M. Brillouin, il ne faut pas se hâter de se féliciter de la concordance qui existe entre les résultats des deux auteurs. Ils ont, en réalité, des sens différents, et on pourrait même être tenté de les regarder comme presque contradictoires.

L'analyse de M. Boltzmann † repose essentiellement sur l'intervention des *chocs :* la condition nécessaire et suffisante pour qu'elle soit valable est que ces chocs se produisent en grand nombre : il en résulte même qu'elle peut être applicable dans un intervalle de temps très court, si le nombre des molécules est assez considérable pour cela.

Gibbs ne considère pas les chocs : nous sommes arrivé au XII[e] Chapitre de son ouvrage sans qu'il y ait fait la moindre allusion. Ses démonstrations concernent des ensembles composés de systèmes sans action les uns sur les autres, ensembles *différents*,

* Ou du moins, dont la densité n'est fonction que de la distance au centre.
† Voir Leçons sur la théorie des gaz, tome I.

par conséquent, de ceux de Boltzmann. Elles font intervenir, au lieu des chocs, des phénomènes de mélange analogues à ceux auxquels nous avons fait allusion tout-à-l'heure pour le cas des liquides. Quelque soit le nombre des systèmes, elles ne s'appliquent qu'à un intervalle de temps $t' - t$ relativement long, assez long pour que deux systèmes S très voisins au temps t puissent correspondre, au temps t', à des points représentatifs très différents, ou inversement.

Malgré cela, nous verrons plus loin que l'identité des deux conclusions n'est probablement pas fortuite, qu'elle est, au contraire, dans la nature des choses. Mais ceci nous amène à un sujet important, aux objections qu'à l'exemple de M. Brillouin * beaucoup de physiciens ont opposées à ces idées de Gibbs et de Boltzmann.

Il est reproché tout d'abord à la démonstration de M. Boltzmann de ne considérer l'effet des chocs que sur les vitesses et de supposer constamment l'homogénéité parfaite en ce qui regarde les positions, alors que cette homogénéité — à supposer qu'elle existât à un instant quelconque — serait manifestement détruite aux instants suivants par le mouvement lui-même. On introduirait ainsi constamment l'inorganisation parfaite (à savoir, l'homogénéité) à un point de vue et c'est ainsi qu'au bout du compte cette inorganisation paraîtrait être la tendance générale du mouvement, tendance qui n'existerait nullement en réalité. En un mot, l'analogie de Boltzmann, radicalement fausse, ne serait pas une réponse à la contradiction signalée plus haut.

M. Brillouin écarte à plus forte raison le raisonnement de Gibbs, image plus grossièrement approchée de la réalité que celui de Boltzmann et qui même, nous l'avons vu, porte sur des phénomènes essentiellement différents.

Je serais, pour ma part, tenté de donner raison à Gibbs et à Boltzmann contre mon savant ami M. Brillouin. Je vais essayer ici d'expliquer pourquoi et de faire à sa critique pénétrante une réponse que le lecteur ne s'étonnera sans doute pas de trouver longue.

Remarquons d'abord que la question est double. La conclusion contestée se compose de deux parties, une négative et une positive. Elle exprime :

* Notes pour la traduction française des Leçons sur la théorie des gaz de Boltzmann, Paris, Gauthier-Villars, 1901–05.

1° que l'objection tirée de la réversibilité des équations du problème n'est pas valable, qu'elle ne prouve pas l'impossibilité de lois finales irréversibles ;

2° qu'en fait, de telles lois irréversibles existent et qu'une certaine quantité (dont on donne l'expression) va en général en croissant.

Or, tant qu'on se borne à affirmer la première partie c'est-à-dire à prétendre que la seconde est *possible*, il ne paraît guère y avoir de doute. Dans ce cas, en effet, il importe peu que les raisonnements présentés correspondent à la réalité physique ; il suffit qu'ils portent sur des phénomènes logiquement concevables. Dès lors, les déductions de Gibbs et de Boltzmann ne sont pas, à ce point de vue, atteintes par les objections précédentes. On peut, au reste, se dispenser d'examiner longuement la question en recourant aux exemples les plus simples, tels que celui des projectiles, la pesanteur étant considérée comme un champ de forces constant en grandeur et en direction. Là encore, les équations du problème sont réversibles ; et cependant si un certain nombre de tels projectiles sont lancés *au hasard*, qui doutera que d'une manière globale, ils n'aillent en descendant ?

Soit encore un système sans forces accélératrices dépendant de deux paramètres ρ, θ, et dont la force vive est donnée, par

$$2T = f(\rho)\left(\frac{d\rho}{dt}\right)^2 + \phi(\rho)\left(\frac{d\theta}{dt}\right)^2.$$

Supposons que $f(\rho)$, mais non $\phi(\rho)$, soit très grand pour ρ voisin d'une certaine valeur ρ_0 et très petit partout ailleurs. Alors toute trajectoire de ce système restera, sauf pendant des intervalles de temps très petits et très éloignés, au voisinage de la ligne $\rho = \rho_0$. Dès lors la différence $|\rho - \rho_0|$ sera en général décroissante pour tout intervalle de temps non très petit, si elle n'est pas initialement très petite. Il est clair qu'un tel résultat n'est irréversible qu'en apparence ; il tient évidemment à ce que les régions du plan (ρ, θ) non très voisines de $\rho = \rho_0$ doivent être considérées comme très petites au point de vue du problème actuel : il y aurait, autrement dit, lieu de définir ici, d'une manière plus ou moins analogue à celle qui a été mentionnée plus haut, des *régions dynamiquement équivalentes* et cette définition serait très différente de la définition géométrique. Nous trouverons d'ailleurs plus loin le point précis par lequel pèche, dans des exemples de cette espèce, l'objection générale.

Employons auparavant une autre comparaison plus lointaine en apparence, beaucoup plus exacte cependant à mon avis. Considérons ce qui se passe lorsqu'on bat un jeu de cartes. C'est ce que l'on fait le plus généralement par une série de coupes successives, l'opération élémentaire consistant à diviser le jeu, d'une manière arbitraire, en trois parties et à intervertir l'ordre de deux consécutives d'entre elles. Il est clair qu'une telle manière de procéder est parfaitement réversible. Cependant il est non moins évident qu'elle tend à une certaine régularisation du jeu. Si, par exemple, une fois les cartes battues nous trouvions toutes les rouges d'abord et toutes les noires ensuite, nous croirions à une fraude. On peut mesurer cette régularisation par un nombre. Considérons, pour fixer les idées, le nombre V de cartes rouges suivies d'une noire. Il est aisé de calculer la valeur moyenne V_0 de V pour tous les ordres possibles dans lesquels les cartes peuvent être rangées. Si maintenant, on part d'un jeu donné pour lequel V a une valeur déterminée V', la valeur probable de ce nombre après battage se rapprochera de V_0. J'entends par là que si au lieu d'un seul jeu nous en considérons un très grand nombre, tous identiques entre eux initialement, mais battus de toutes façons possibles et au hasard, la moyenne des valeurs de V pour ces différents jeux sera comprise entre V' et V_0.

Cette opération du battage est tout à fait comparable aux phénomènes qui ont lieu pour nos systèmes S, même lorsqu'on reste au point de vue de Gibbs. La position P' d'un point représentatif au temps t' étant une fonction *pratiquement discontinue* de la position P du même point au temps t, lorsque l'intervalle $t' - t$ est très long (c'est-à-dire qu'un changement très petit de P produit un changement très grand de P'), tout se passe comme si, P étant donné, P' était plus ou moins indéterminé, comme si la donnée de P équivalait au lancé d'une roulette dont l'arrêt marquerait la position de P'. Cette roulette fonctionnerait aussi parfaitement que possible si l'indétermination de P' était absolue (sauf la condition $\varepsilon = \text{const.}$)

C'est l'hypothèse dont nous avons parlé précédemment et sur le bien fondé de laquelle on ne peut se prononcer avec certitude. Mais même si elle était inexacte, il n'en est pas moins vrai que des points représentatifs primitivement rapprochés les uns des autres se dispersent avec le temps et que notre ensemble de systèmes S est ainsi " battu."

Il est clair également que les chocs agissent dans le même

sens et d'une manière beaucoup plus efficace encore. Nous voyons maintenant comment les procédés si différents de Gibbs et de Boltzmann peuvent conduire au même résultat. C'est ainsi que la valeur moyenne du nombre V considéré plus haut tendrait encore vers V_0 si l'on employait pour battre les cartes, au lieu du procédé auquel nous avons fait allusion, un autre quelconque de ceux qui sont usités dans ce but.

Ceci tient, remarquons-le, à ce que ces procédés si différents qu'ils puissent être, ne favorisent pas une permutation plutôt qu'une autre. Il faudrait donc savoir de même dans le cas de nos systèmes S, quelles sont les distributions également favorisées et leur définition dépendra du mode de "battage." Toutefois l'exemple même qui précède montre que cette définition pourra être la même pour des modes de battage très différents.

Passons à la seconde partie de la thèse, à la question de savoir si, en effet, la valeur moyenne de l'indice de probabilité changé de signe est en général croissante.

Du moment que M. Boltzmann est obligé d'attribuer aux molécules d'autres positions que celles qu'elles ont en réalité, son raisonnement doit, si l'on veut prendre les choses à la lettre, être considéré comme inexistant.

Disons tout de suite que Gibbs, de son côté, n'apporte pas de démonstration rigoureuse.

Non seulement il admet l'absence de chocs, mais — si j'ai bien compris son raisonnement — il ne tient pas suffisamment compte du phénomène même de mélange sur lequel il se base. Ce phénomène a, en effet, pour résultat, me semble-t-il, de rendre l'application de la loi des grands nombres aux extensions en phase infiniment petites (fondement premier de toute cette analyse) sinon douteuse du moins plus délicate que la brève rédaction de Gibbs ne le comporterait. La conclusion même serait de nature à faire mettre en doute la légitimité de la démonstration, en vertu du principe "qui veut trop prouver ne prouve rien." Elle n'admet, en effet, d'autres cas d'exception que ceux où il n'y a pas mélange, où les équations différentielles données définissent des trajectoires toutes stables au sens de M. Liapounoff: l'inégalité qui exprime le théorème serait alors remplacé par une égalité. Or il y a lieu, en l'espèce, de prévoir des cas d'exception beaucoup plus étendus, correspondant non à des formes particulières des équations différentielles, mais à certaines distributions initiales des systèmes S, et pour lesquels l'inégalité serait *renversée*.

Mais si nous ne nous autorisons pas des raisonnements de Gibbs et de Boltzmann pour énoncer une affirmation rigoureuse, quelles présomptions ces raisonnements créent-ils ? S'ils sont défectueux dans quel sens pèchent-ils ?

Devons-nous, par exemple, croire avec M. Brillouin qu'en introduisant l'homogénéité, on introduit du même coup l'inorganisation ? Je ne le pense pas. Le sens du mot " inorganisé " demanderait à être défini ; mais, en tout cas, la distribution la plus " inorganisée " possible n'est pas plus la distribution homogène que n points pris au hasard sur une circonférence n'ont de chance pour être les sommets d'un polygone régulier. Pour revenir à notre comparaison de tout à l'heure, un jeu de cartes, une fois battu, où les rouges seraient toutes réunies ainsi que les noires, serait suspect ; mais un jeu où il y aurait alternance parfaitement régulière entre les uns et les autres ne le serait pas moins. Si — ce qui est certain — une distribution homogène des systèmes S dans l'espace à n dimensions à un instant t est remplacée aux instants suivants par une distribution non homogène, celle-ci est, selon toute vraisemblance, beaucoup plus inorganisée que la première. Non seulement, si erreur il y a, c'est dans le sens de l'organisation, mais on pouvait être presque sûr à priori, qu'il en serait ainsi. Une erreur de cette espèce a, en effet, toujours pour origine le désir de simplifier le raisonnement. Or simplifier, en mathématique, c'est débrouiller un peu le chaos des hypothèses possibles : en un mot, c'est organiser.

Quant à une définition satisfaisante de l'inorganisation, elle est évidemment difficile à donner. Je proposerai, faute de mieux, l'une ou l'autre des deux suivantes.

On peut d'abord * considérer comme type de distribution inorganisée l'ensemble de toutes les distributions possibles des systèmes S. Une distribution déterminée D serait alors dite inorganisée si elle ne se distinguait en rien, *au point de vue de la question étudiée*, de la distribution totale dont nous venons de parler. On pourrait donc dans le raisonnement substituer cette dernière à la distribution considérée, — par exemple, prendre pour valeur d'une quantité qui dépend de la distribution D, sa valeur moyenne dans toutes les distributions possibles — et c'est la possibilité de cette substitution, qui exprimerait le caractère inorganisé de D.

Dans cette conception, une distribution pourrait — conformé-

* C'est ce que propose Gibbs à la fin du chap. XI (p. 251).

ment à la nature des choses — être considérée comme inorganisée relativement à telle question et comme organisée relativement à telle autre. Cela peut être le cas, par exemple, pour la distribution homogène (en position) des systèmes S: il est possible que l'organisation ainsi introduite soit sans influence dans la question qui nous occupe.

On peut aussi admettre simplement que les distributions "organisées" sont des distributions *exceptionnelles*, au sens adopté par M. Poincaré à propos de son théorème sur la stabilité à la Poisson. L'hypothèse, faite au cours du raisonnement, que le mouvement est inorganisé, reviendrait alors à dire, sans plus préciser, que le théorème est vrai sauf pour certaines distributions exceptionnelles.

L'une ou l'autre des deux définitions précédentes suppose d'ailleurs la connaissance de ce que l'on doit appeler distributions "également probables." Toutefois, on sait que la notion de distributions exceptionnelles est en somme indépendante de la convention adoptée à cet égard.

Mais la notion de mouvement inorganisé absolument parlant, n'est pas la seule qu'il convienne d'introduire. Il y a lieu d'y joindre, suivant un procédé classique en calcul des probabilités, celle de mouvement inorganisé parmi ceux qui satisfont à des conditions données.

L'adaptation, à cet effet, des deux définitions données tout à l'heure ne souffre évidemment aucune difficulté. Si j'insiste sur cette extension c'est qu'elle permet à mon sens de mettre en évidence le véritable vice de l'objection que je m'étais proposé de discuter.

Celle-ci repose, en effet, sur ce fait, que tout mouvement qui a existé du temps t au temps postérieur t' peut être renversé, le temps t' étant considéré comme antérieur à t et que, *inorganisé dans le premier cas, il le sera également* dans le second.

C'est ce dernier point qui ne me paraît pas exact.

Reprenons le cas des projectiles pesants, en supposant donnée, pour fixer les idées, la constante des forces vives. Considérons une série de ces projectiles lancés au hasard à une même hauteur h et dont, par conséquent, les mouvements ne seront pas exceptionnels, au moins à ce moment-là. Tous passeront ensuite à la hauteur h', celle-ci étant supposée inférieure, et inférieure d'une quantité très grande, à h. Mais, dans cette nouvelle situation de nos projectiles, leurs mouvements seront devenus exceptionnels : ils seront tous sensiblement verticaux et descendants.

L'explication du paradoxe est immédiate : au premier instant, nos mobiles ne sont pas exceptionnels parmi ceux qui sont à cet instant-là à la hauteur $z = h$; au second instant, ils sont exceptionnels parmi ceux qui (toujours avec la vitesse correspondante à la valeur donnée de la constante des forces vives) sont à la hauteur h' : question toute différente de la première.

Autrement dit, le fait, pour un mouvement, d'être organisé ou inorganisé dépend de ce que l'on suppose préalablement connu à son sujet.

Si donc H est une certaine fonction de la distribution des systèmes S, la propriété suivante :

"La quantité H est croissante si la distribution est inorganisée"

est susceptible d'un énoncé du genre de celui-ci, où je ne vois plus de prise pour l'objection de réversibilité :

"Soient H_1 et $H_2 < H_1$ deux valeurs de H ; T un intervalle de temps convenablement choisi.

"Désignons par M_1 les mouvements pour lesquels, pendant l'intervalle T, la quantité H prend au moins une fois la valeur H_1 ; par M_2, les mouvements pour lesquels (au cours du même intervalle de temps) H prend au moins une fois la valeur H_2 ; par M_3, ceux qui satisfont à l'une et à l'autre des deux conditions précédentes ; autrement dit, qui sont à la fois des mouvements M_1 et des mouvements M_2.

"Les M_3 sont exceptionnels parmi les M_1, mais non parmi les M_2."

Je ne doute guère, pour ma part, de l'existence d'un pareil énoncé après les déductions de Gibbs et de Boltzmann ; et si leur conclusion était erronée, l'erreur ne me paraîtrait pouvoir porter que sur l'expression de la quantité H.

Je me suis laissé entraîner à commenter longuement cette partie si essentielle de l'oeuvre de Gibbs, et je craindrais d'abuser de l'attention du lecteur en insistant sur les trois derniers chapitres.

Nous avons, au reste, déjà parlé du sujet — le plus important peut-être après celui, qui vient de nous occuper — auquel se rapportent les chapitres XIII et XIV : les définitions de la température et de l'entropie, dans l'hypothèse cinétique : définitions qui sont examinées — toujours, il est vrai, en admettant la distribution canonique ou microcanonique, à la lumière des principales propriétés des deux notions à définir.

Le dernier chapitre de l'ouvrage mériterait une attention spéciale. Gibbs y considère des ensembles ("grands ensembles") dont les systèmes constituants sont eux-mêmes des ensembles ("petits ensembles") de molécules. Le nombre de ces molécules varie d'ailleurs d'un "petit ensemble" à un autre, comme leur positions et leurs vitesses; et, d'autre part, on admet l'échange des particules entre les différents "petits ensembles." C'est dans les phénomènes de diffusion, particulièrement dans ceux qui ont lieu au contact des membranes semi-perméables, que Gibbs fait intervenir cette nouvelle catégorie d'ensembles, laquelle présente, on le voit, un caractère nouveau et important au point de vue théorique, puisque les différents systèmes partiels cessent d'être entièrement indépendants.

Telle est, dans ses grandes lignes, la dernière oeuvre de Gibbs. Il ne pourrait en exister de plus actuelle et de plus importante, en dépit de la modestie de son titre. Cette modestie n'est d'ailleurs pas de pure forme: elle correspond bien à la pensée arrêtée de l'auteur. Gibbs, nous l'avons vu, n'a pas abordé la théorie des chocs: il a voulu, sauf en son dernier chapitre, se borner au cas le plus simple, celui où le mouvement d'un des systèmes n'est pas troublé par la présence des autres. Son but n'a pas été, en un mot, de remplacer des ouvrages comme les Leçons sur la théorie des gaz de Boltzmann, mais de leur servir de base, ainsi qu'aux autres études qu'on pourra entreprendre sur le même sujet. C'est en ce sens surtout, peut-être, qu'il a fait oeuvre de mathématicien; cette sorte de besogne préparatoire est, il faut nous l'avouer, la seule qui nous incombe le plus souvent. Encore si nous pouvions la mener assez vite pour fournir aux besoins des sciences de la nature; mais rien que dans les Principes élémentaires de mécanique statistique, combien de questions avons-nous rencontrées auxquelles notre théorie des équations différentielles est actuellement incapable de répondre! Cependant pour effacé qu'il soit, ce rôle n'en est pas moins essentiel. Les successeurs de Boltzmann ne pourront se dispenser d'utiliser l'ouvrage de Gibbs: lui seul, en maintes circonstances, peut leur assurer l'instrument commode — et, avant tout, l'instrument sûr, dont ils ont besoin.

JACQUES HADAMARD.

NOTES.

AT the annual meeting of the London mathematical society held on November 9, the following officers were elected: A. R. FORSYTH, president; A. E. H. LOVE and J. H. GRACE, secretaries; J. LARMOR, treasurer; two vice-presidents and ten other members of the council. The following papers were read: By Mr. G. H. HARDY, "The continuum and the second number class"; by Professor W. BURNSIDE, "On the arithmetic nature of the coefficients in a group of linear substitutions of finite order (second paper)"; by Mr. J. W. NICHOLSON, "On the asymptotic value of a type of finite series"; by Professor T. J. I'A. BROMWICH, "On the extension of Dirichlet's integral"; by Dr. E. W. HOBSON, "On improper multiple integrals," and "On the arithmetic continuum."

AT the Meran meeting of the German mathematical society Professor A. PRINGSHEIM was elected president, and Professor A. KRAZER was re-elected secretary. Professors E. STUDY, of Bonn, and A. v. BRILL, of Tübingen were elected to the council. The society now numbers 666 members.

THE annual meeting of the association of teachers of mathematics of the Middle States and Maryland was held on Saturday, December 2, at Annapolis, Md., in affiliation with the association of colleges and preparatory schools of the Middle States and Maryland. The following papers were read: "How should the college teach analytic geometry?" by Professor H. S. WHITE; "Suggestions for the first twelve lessons in demonstrative geometry," by Mr. H. R. HIGLY; "Some essentials of the successful mathematics teacher," by Dr. J. S. FRENCH; "The teaching of geometry," by Dr. H. A. CONVERSE.

The officers elected for the coming year were: President, Professor E. S. CRAWLEY, University of Pennsylvania; Vice-president, Dr. J. S. FRENCH, Jacob Tome Institute, Port Deposit, Md.; Secretary and Treasurer, Dr. J. T. RORER, Central High School, Philadelphia, Pa. The association adopted the following resolution:

That this association approve of the organization of a national federation of existing associations of teachers of mathematics in which each association shall preserve its own

organization and individuality and which shall have among its objects the joint support of publication. In the federation should be included only societies representing territory as extensive at least as one state.

THE fifth annual meeting of the central association of science and mathematics teachers was held at Chicago December 1 and 2. The following papers in mathematics were read: "The straight line in geometry," by J. W. WITHERS; "Interest and progress in the teaching of mathematics," by N. J. LENNES; "Aims in teaching algebra," by R. J. ALEY; "Some thoughts on the teaching of geometry," by C. A. PETTERSON.

THE first award of the new Bolyai prize recently founded by the Hungarian academy of sciences (see current volume of BULLETIN, page 43) was made to Professor H. POINCARÉ. The works of Professor D. HILBERT not only received honorable mention, but the report of the commission will contain a detailed account of his memoirs, as well as of those of the winner of the prize.

THE Munich technical school announces the following prize problem: "To develop historically the theory of the singular solutions of total differential equations of the first order from the first appearance of the theory to the present time. For the older theory, the memoir of Houtain, 'Des solutions singulières des équations différentielles' (*Annales des Universités de Belgique*, années 1851–1854) is to be critically discussed." Competing memoirs should be sent to the Senat der Technischen Hochschule zu München before October 15, 1906.

THE proceedings of the international congress of arts and sciences held at St. Louis in September, 1904, are being published by Houghton, Mifflin and Company in eight volumes, the first of which will contain the papers in philosophy and mathematics. The papers will be published as presented, except that those read in foreign languages will be translated into English.

WITH the appearance of its fifteenth volume, the publication of the *Jornal de Sciencias mathematicas e astronomicas*, edited by Professor F. G. TEIXEIRA, has been discontinued. In its place appears, under the direction of Professor Teixeira, the *Annaes Scientificos da Academia Polytechnica do Porto*, of which the first number has just been issued.

THE firm of Macmillan and Bowes, 1 Trinity St., Cambridge, England, announces that a series of short tracts of from 30 to 70 pages on various topics of pure mathematics and theoretical physics is soon to appear, edited by Mr. J. G. LEATHAM and Mr. E. T. WHITTAKER. The chief purpose is to assist in the maintenance of a high standard in mathematical teaching by the infusion of new methods and by the extension of knowledge of recent research. The following will shortly be published: Surface and volume integrals in physics, by J. G. LEATHAM; The integration of functions of a single variable, by G. H. HARDY; Quadratic forms and their classification by means of invariant factors, by J. I'A. BROMWICH; Definite integral, meaning and properties, by E. W. HOBSON; Singular points and asymptotes of plane curves, by C. A. SCOTT; The axioms of geometry, by A. N. WHITEHEAD; The eikonal and its application to optical instruments, by E. T. WHITTAKER.

THE publishing house of G. J. Göschen in Leipzig announces the following extensions of the Sammlung Schubert: "Theorie der ebenen algebraischen Kurven höherer Ordnung," by Dr. H. WIELEITNER, which appeared in November, and "Gewöhnliche Differentialgleichungen beliebiger Ordnung," by Professor J. HORN, which will be ready soon. A second volume on plane curves, treating special and transcendental curves has been promised by Dr. Wieleitner.

THE following university courses in mathematics are announced:

CAMBRIDGE UNIVERSITY. — *Michaelmas term*, 1905. — By Professor A. R. FORSYTH: Partial differential equations, three hours. — By Professor Sir G. H. DARWIN: Theory of potential and attractions, three hours. — By Professor Sir R. S. BALL: Planetary theory, three hours. — By Professor J. LARMOR: Electricity and magnetism, three hours. — By Professor J. J. THOMSON: Properties of matter, three hours; Electricity and matter, two hours. — By Professor B. HOPKINSON: Applied mathematics, two hours; Electricity, two hours. — By Dr. E. W. HOBSON: Vibrations and sound, three hours. — By Dr. H. F. BAKER: Introduction to theory of functions, three hours; Solid geometry, three hours. — By Mr. H. W. RICHMOND: Analytic geometry, three hours. — By Mr. E. T. WHITTAKER: Theory of optical instruments, three hours. — By Dr. A. N. WHITEHEAD: Principles of mathematics. — By

Mr. A. BERRY: Elliptic functions, Bessel functions and Fourier series, three hours. — By Mr. A. MONRO: Hydrodynamics and sound, three hours. — By Mr. J. G. LEATHEM: Elementary electron theory, three hours. — By Mr. J. H. GRACE: Invariants and geometric applications, three hours. — By Mr. E. W. BARNES: Gamma functions, three hours.

Lent term, 1906. — By Professor A. R. FORSYTH: Partial differential equations, II., three hours. — By Professor Sir G. H. DARWIN: Dynamical astronomy (elementary), three hours. — By Professor Sir R. S. BALL: An elementary course on quaternions, three hours. — By Professor J. LARMOR: Electrodynamics with optical applications, three hours. — By Professor J. J. THOMSON: Electricity and magnetism, three hours; Discharge of electricity through gases, two hours. — By Professor B. HOPKINSON: Applied mathematics II, two hours; Electricity II, two hours. — By Dr. E. W. HOBSON: Harmonic analysis, three hours. — By Dr. H. F. BAKER: Theory of functions, three hours; Analysis, three hours. — By Mr. E. T. WHITTAKER: The differential equations of applied mathematics, three hours. — By Mr. H. W. RICHMOND: Analytical geometry, three hours. — By Mr. R. A. HERMAN: Hydrodynamics, two courses. — By Dr. A. N. WHITEHEAD: Symbolic logic and its applications to mathematics. —By Mr. A. BERRY: Elliptic functions, three hours. — By Mr. C. T. BENNETT: Line geometry, three hours. — By Mr. E. W. BARNES: Linear differential equations, three hours.

Easter term, 1906. — By Professor A. R. FORSYTH: Partial differential equations III, three hours. — By Professor J. LARMOR: Theory of gases and thermodynamics, two hours. — By Professor J. J. THOMSON: Electricity and magnetism, three hours. — By Dr. E. W. HOBSON: Theory of the continuum, three hours. — By Dr. H. F. BAKER: Theory of functions, three hours; Analysis, three hours. — By Mr. W. L. MOLLISON: Theory of potential and electrostatics, three hours. — By Dr. A. N. WHITEHEAD: Non-euclidean geometry. — By Mr. A. BERRY: Transformation of elliptic functions. — By Mr. G. H. HARDY: Integral functions.

Long vacation, 1906.—By Mr. H. W. RICHMOND: Geometry, three hours. — By Mr. W. M. COATES: Electricity and magnetism. — By Mr. J. G. LEATHEM: Physical optics, three hours. — By Mr. A. YOUNG: Theory of invariants, three hours.

PROFESSOR C. J. DE LA VALLÉE-POUSSIN, of the University of Louvain, has been awarded the Belgian decennial prize in mathematics.

DR. K. CARDA, of the University of Vienna, has been appointed associate professor of mathematics at the technical school in Vienna.

DR. M. DEHN, of the University of Münster, has been promoted to an associate professorship of mathematics.

PROFESSOR E. DOLEZAL, of Leoben, has been appointed professor of practical geometry at the technical school in Vienna.

PROFESSOR F. HAHN, of the school of engineering at Lausanne, has been appointed professor of applied mathematics at the University of Nancy.

DR. G. HAMEL, of the technical school at Karlsruhe, has been appointed professor of mechanics at the German technical school in Brünn, Austria.

PROFESSOR C. KOEHLER, of the University of Heidelberg, has been promoted to a full professorship of mathematics.

PROFESSOR A. SOMMERFELD, of the technical school at Aachen, has been decorated with the order of the red eagle of the fourth class.

DR. J. WEINGARTEN, of the technical school at Berlin, has been appointed honorary professor of mathematics at the University of Freiburg.

PROFESSOR W. v. DYCK, of the technical school at Munich, has been decorated with the Prussian order of the crown of the second class.

MR. F. S. PINKERTON has been appointed professor of applied mathematics at the University of South Wales, Cardiff, Wales.

DR. R. BAIRE has been appointed instructor in pure mathematics at the University of Dijon.

DR. P. BOUTROUX has been appointed master of conferences in mathematics at the University of Montpellier.

MR. TH. FRIESSENDORFF has been appointed docent in applied mathematics at the University of St. Petersburg.

DR. R. FUËTER has been appointed docent in mathematics at the University of Marburg.

DR. E. LANDAU, of the University of Berlin, has been promoted to an associate professorship of mathematics at the same institution.

DR. E. PRINGSHEIM, of the University of Berlin, has been appointed professor of theoretical physics at the University of Breslau.

PROFESSOR J. RUIS CASTIZO, of the University of Zaragoza, has been appointed professor of theoretical mechanics at the University of Madrid.

PROFESSOR K. SCHWARZSCHILD, of the University of Göttingen, has been elected to membership in the royal society of Göttingen.

PROFESSOR K. HELLMER, of the technical school at Brünn (Austria) and PROFESSOR A. SCHELL, of the technical school at Vienna, have retired, each with the honorary title of Hofrat.

AT the Paris Ecole polytechnique, Professor G. KOENIGS has been appointed "répétiteur" in analysis; Dr. R. BRICARD and Dr. M. FOUCHÉ to the same position in geometry.

AT the University of Kansas, Professor H. B. NEWSON has been promoted to a full professorship of mathematics, and Mr. J. A. G. SHIRK has been appointed instructor in mathematics.

THE following changes have been announced at the University of Wisconsin: Mr. W. M. PERSONS has resigned his instructorship in mathematics to become assistant professor of statistics at Dartmouth College; Mr. E. R. SMITH, Mr. E. A. MORITZ, Mr. R. S. PEATTER and Dr. D. A. LEHMAN have been appointed instructors in mathematics.

MISS S. E. CRONIN, Mr. J. SUTER, and Mr. G. ANDERSON have been appointed instructors in mathematics at the Iowa State College, Ames, Iowa.

PROFESSOR J. E. BONEBRIGHT, of the Colorado Agricultural College, has been appointed professor of mathematics at Ottawa University, Ottawa, Kansas.

DR. MAX MASON has been promoted to an assistant professorship in mathematics in the Sheffield Scientific School, Yale University.

MR. G. H. LIGHT and Mr. W. A. TEHRING have been appointed instructors in mathematics at Purdue University.

MR. H. W. STAGER and Mr. T. G. BROWN have been appointed assistants in mathematics at Stanford University.

PROFESSOR OTTO STOLZ, of the University of Innsbruck, died November 23, at the age of sixty-three years. Professor VICTOR SCHLEGEL, of the Höhere Maschinenbauschule at Hagen, died November 22 at the age of sixty-three years. Professor Schlegel had been a member of the AMERICAN MATHEMATICAL SOCIETY since 1894.

THE following catalogues of second-hand books on mathematics and physics have recently appeared: A. Hermann, 6 rue de la Sorbonne, Paris V, catalogue no. 86, containing 4073 titles in mathematics; Macmillan and Bowes, 1 Trinity St., Cambridge, England, special sale list of works on applied mathematics; Gustav Fock, Schlossgasse 7, Leipzig, hand apparatus of the late Professor Guido Hauck, 1960 dissertations and reprints; Ernst Carlebach, Heidelberg, catalogue no. 276, 842 entries in science and mathematics; Martinus Nijhoff, the Hague, Nobelstraat 18, catalogue no. 342, containing 1589 titles in exact sciences.

THE library of the late Professor Luigi Cremona, of Rome, is offered for sale. It consists of 3657 reprints and monographs in Italian, 1163 in French, 995 in German, 509 in English and 23 in Latin. The list of books is, however, very small. There are 231 books in Italian, 208 in French, 209 in German, 88 in English, and 18 in Latin. There are also a number of other entries in the catalogue relating to miscellaneous memoirs and periodicals.

NEW PUBLICATIONS.

I. HIGHER MATHEMATICS.

AMSLER-LAFFON (J.). Zur Lebensgeschichte von Franz Neumann (1798–1895). Zürich, 1904. 8vo. 16 pp. M. 1.00

BAILLAUD (B.). See HERMITE.

BALL (W. W. R.). Histoire des mathématiques. Traduction française, sur la 3e édition anglaise, avec de nombreuses notes et d'importantes additions. Vol. I: Depuis les temps les plus reculés jusqu'à Newton. Paris, Hermann, 1905. 8vo. 425 pp. F. 12.00

BECK (H.). Die Strahlenketten im hyperbolischen Raum. (Diss.) Bonn, 1905. 8vo. 55 pp.

BOEGEHOLD (F.). Ableitung einiger Eigenschaften des Kegelschnittbüschels aus orthogonalen Invarianten und Covarianten. Jena, 1905. 8vo. 78 pp. M. 2.00

BOURGET (H.). See HERMITE.

BÜRKLEN (O. T.). Aufgabensammlung zur analytischen Geometrie der Ebene. Leipzig, Göschen, 1905. 16mo. 196 pp. Cloth. (Sammlung Göschen, No. 256.) M. 0.80

GOURSAT (E.). Cours d'analyse mathématique. Vol. II: Théorie des fonctions analytiques; équations différentielles; équations aux dérivées partielles; éléments du calcul des variations. Paris, Gauthier-Villars, 1905. 8vo. 6 + 640 pp. (Cours de la Faculté des sciences de Paris.) Fr. 20.00

—— A course in mathematical analysis. Translated by E. R. Hedrick. Vol. I: Derivatives and differentials; definite integrals; expansion in series; applications to geometry. Boston, Ginn [1904]. 8vo. 8 + 548 pp. Cloth. $4.00

HALLER (S.). Ueber die Brennpunktskurven zweier besonderer Gruppen von Kegelschnittsystemen. (Progr.) München, 1905. 8vo. 31 pp.

HARTWIG (J.). Die Sätze von Pascal und Brianchon auf der Kugelfläche. (Progr.) Wiener-Neustadt, 1905. 8vo. 11 pp.

HEBERDEY (P.). Die Kegelschnitte. (Progr.) Wien, 1905. 8vo. 35 pp.

HEDRICK (E. R.). See GOURSAT (E.).

HERMITE et STIELTJES. Correspondance d'Hermite et de Stieltjes, publiée par les soins de B. Baillaud et H. Bourget, avec une préface de E. Picard. Vol. II: 18 octobre 1889 — 15 décembre 1894. Paris, Gauthier-Villars, 1905. 8vo. 6 + 466 pp. Fr. 16.00

ISSALY. La cinématique dans les rapports, absolus avec les pseudo-surfaces, et conditionnels avec les surfaces. Etude servant d'application aux "Principes fondamentaux de la théorie des pseudo-surfaces" du même auteur et de son mémoire "La géométrie non-euclidienne et l'insuffisance de ses principes." Paris, Hermann, 1905. 8vo. 59 pp. F. 3.00

KEYSER (C. J.). Concerning certain 4-space quintic configurations of point ranges and congruences, and their sphere analogues in ordinary space. 4to. (*American Journal of Mathematics* 27, pp. 303–314.)

KOMMERELL (K.). Riemannsche Flächen im ebenen Raum von vier Dimensionen. (Progr.) Heilbronn, 1905. 8vo. 49 pp. M. 1.60

LEONARD (H. B.). On the definition of reducible hypercomplex number systems. II. 4to. (*American Journal of Mathematics* 27, pp. 381–405.)

LOREY (W.). Ueber die Wohltat und das Werden der Zahl. (Progr.) Görlitz, 1905. 4to. 10pp.

MATEU (A. F.). Cálculo infinitesimal. Vol. I: Nociones generales abstractas de cálculo diferencial. Oviedo, 1904. 4to. 120 pp. F. 10.00

NEIKIRK (L. I.). Groups of order p^m which contain cyclic subgroups of order p^{m-2}. 8vo. 65 pp. (*Publications of the University of Pennsylvania*, series in mathematics, No. 3.)

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